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ARTICLES

American Political Science Review Vol. 87, No. 3 September 1993

BEYOND TOCQUEVILLE, MYRDAL, AND HARTZ: THE MULTIPLE TRADITIONS IN AMERICA

ROGERS M. SMITH *Yale University*

Analysts of American politics since Tocqueville have seen the nation as a paradigmatic "liberal democratic" society, shaped most by the comparatively free and equal conditions and the Enlightenment ideals said to have prevailed at its founding. These accounts must be severely revised to recognize the inegalitarian ideologies and institutions of ascriptive hierarchy that defined the political status of racial and ethnic minorities and women through most of U.S. history. A study of the period 1870–1920 illustrates that American political culture is better understood as the often conflictual and contradictory product of multiple political traditions, than as the expression of hegemonic liberal or democratic political traditions.

Since the nation's inception, analysts have described American political culture as the preeminent example of modern liberal democracy, of government by popular consent with respect for the equal rights of all. They have portrayed American political development as the working out of liberal democratic or republican principles, via both "liberalizing" and "democratizing" socioeconomic changes and political efforts to cope with tensions inherent in these principles. Illiberal, undemocratic beliefs and practices have usually been seen only as expressions of ignorance and prejudice, destined to marginality by their lack of rational defenses. A distinguished line of writers, from Hector St. John Crevecoeur in the eighteenth century and Harriet Martineau and Lord Bryce in the nineteenth century to Gunnar Myrdal and Louis Hartz in the twentieth century serves as authority for this view. Today, leading social scientists such as Samuel P. Huntington, Walter Dean Burnham, and Ira Katznelson, legal scholars, historians, and cultural analysts such as Kenneth Karst, John Diggins, and Sacvan Bercovitch, and many others still structure their accounts on these premises. Virtually all appeal to the classic analysis of American politics, Tocqueville's *Democracy in America*.

Tocqueville's thesis—that America has been most shaped by the unusually free and egalitarian ideas and material conditions that prevailed at its founding—captures important truths. Nonetheless, the purpose of this essay is to challenge that thesis by showing that its adherents fail to give due weight to inegalitarian ideologies and conditions that have shaped the participants and the substance of American politics just as deeply. For over 80% of U.S. history, its laws declared most of the world's population to be ineligible for full American citizenship solely because of their race, original nationality, or gender. For at least two-thirds of American history, the majority of the domestic adult population was also ineligible for full citizenship for the same reasons. Contrary to Tocquevillian views of American civic identity, it did not matter how "liberal," "democratic," or "republican" those persons' beliefs were.[1]

The Tocquevillian story is thus deceptive because it is too narrow. It is centered on relationships among a minority of Americans (white men, largely of northern European ancestry) analyzed via reference to categories derived from the hierarchy of political and economic statuses men have held in Europe: monarchs and aristocrats, commercial burghers, farmers, industrial and rural laborers, and indigents. Because most European observers and British American men have regarded these categories as politically fundamental, it is understandable that they have always found the most striking fact about the new nation to be its lack of one type of ascriptive hierarchy. There was no hereditary monarchy or nobility native to British America, and the revolutionaries rejected both the authority of the British king and aristocracy and the creation of any new American substitutes. Those features of American political life made the United States appear remarkably egalitarian by comparison with Europe.

But the comparative moral, material, and political egalitarianism that prevailed at the founding among moderately propertied white men was surrounded by an array of other fixed, ascriptive systems of unequal status, all largely unchallenged by the American revolutionaries.[2] Men were thought naturally suited to rule over women, within both the family and the polity. White northern Europeans were thought superior culturally—and probably biologically—to black Africans, bronze Native Americans, and indeed all other races and civilizations. Many British Americans also treated religion as an inherited condition and regarded Protestants as created by God to be morally and politically, as well as theologically, superior to Catholics, Jews, Muslims, and others.

These beliefs were not merely emotional prejudices or "attitudes." Over time, American intellectual and political elites elaborated distinctive justifications for these ascriptive systems, including inegalitarian scriptural readings, the scientific racism of the "American school" of ethnology, racial and sexual Darwinism, and the romantic cult of Anglo-Saxonism in American historiography. All these discourses identified the true meaning of *Americanism* with particular forms of cultural, religious, ethnic, and especially racial and gender hierarchies.[3] Many adherents of ascriptive Americanist outlooks insisted that the nation's political and economic structures should formally reflect natural and cultural inequalities, even at the cost of violating doctrines of universal rights. Although these views never entirely prevailed, their impact has been wide and deep.

Thus to approach a truer picture of America's political culture and its characteristic conflicts, we must consider more than the familiar categories of (absent) feudalism and socialism and (pervasive) bourgeois liberalism and republicanism. The nation has also been deeply constituted by the ideologies and practices that defined the relationships of the white male minority with subordinate groups, and the relationships of these groups with each other. When these elements are kept in view, the flat plain of American egalitarianism mapped by Tocqueville and others suddenly looks quite different. We instead perceive America's initial conditions as exhibiting only a rather small, recently leveled valley of relative equality nestled amid steep mountains of hierarchy. And though we can see forces working to erode those mountains over time, broadening the valley, many of the peaks also prove to be volcanic, frequently responding to seismic pressures with outbursts that harden into substantial peaks once again.

To be sure, America's ascriptive, unequal statuses, and the ideologies by which they have been defended have always been heavily conditioned and constrained by the presence of liberal democratic values and institutions. The reverse, however, is also true. Although liberal democratic ideas and practices have been more potent in America than elsewhere, American politics is best seen as expressing the interaction of multiple political traditions, including *liberalism, republicanism,* and *ascriptive forms of Americanism,* which have collectively comprised American political culture, without any constituting it as a whole.[4] Though Americans have often struggled over contradictions among these traditions, almost all have tried to embrace what they saw as the best features of each.

Ascriptive outlooks have had such a hold in America because they have provided something that neither liberalism nor republicanism has done so well. They have offered creditable intellectual and psychological reasons for many Americans to believe that their social roles and personal characteristics express an identity that has inherent and transcendant worth, thanks to nature, history, and God. Those rationales have obviously aided those who sat atop the nation's political, economic, and social hierarchies. But many Americans besides elites have felt that they have gained meaning, as well as material and political benefits, from their nation's traditional structures of ascribed places and destinies.

Conventional narratives, preoccupied with the absence of aristocracy and socialism, usually stress the liberal and democratic elements in the rhetoric of even America's dissenters (Hartog 1987). These accounts fail to explain how and why liberalizing efforts have frequently lost to forces favoring new forms of racial and gender hierarchy. Those forces have sometimes negated major liberal victories, especially in the half-century following Reconstruction; and the fate of that era may be finding echoes today.

My chief aim here is to persuade readers that many leading accounts of American political culture are inadequate. I will also suggest briefly how analyses with greater descriptive and explanatory power can be achieved by replacing the Tocquevillian thesis with a *multiple-traditions* view of America. This argument is relevant to contemporary politics in two ways. First, it raises the possibility that novel intellectual, political, and legal systems reinforcing racial, ethnic, and gender inequalities might be rebuilt in America in the years ahead. That prospect does not seem plausible if the United States has always been essentially liberal democratic, with all exceptions marginal and steadily eliminated. It seems quite real, however, if liberal democratic traditions have been but contested parts of American culture, with inegalitarian ideologies and practices often resurging even after major enhancements of liberal democracy. Second, the political implications of the view that America has never been completely liberal, and that changes have come only through difficult struggles and then have often not been sustained, are very different from the complacency—sometimes despair—engendered by beliefs that liberal democracy has always been hegemonic.

I shall review and critique Tocqueville's account of the sources and dynamics shaping democracy in America, along with two of the most influential extensions of Tocquevillian analysis in modern social science, Gunnar Myrdal's (1944) *American Dilemma* and Louis Hartz's (1955) *Liberal Tradition in America.* I argue that Tocqueville himself was much more perceptive than his modern "Tocquevillian" followers, though not free from the problems identified here. I shall note how Tocquevillian premises continue to flaw recent scholarship, especially general works on American political identity and citizenship. Finally, I shall illustrate the merits of a multiple-traditions approach by showing how it offers more insight into the qualified but extensive creation of new systems of ascriptive inequality during the post-Reconstruction and Progressive eras.

THE TOCQUEVILLIAN THESIS

Tocqueville began *Democracy in America* by calling attention to the immense influence of one "basic fact" that was the creative element from which each particular fact—and, indeed, the whole course—of American society is derived, namely, "the equality of conditions." This "fact" absorbed Tocqueville's interest because he saw a democratic revolution taking place in Europe, especially in France, breaking down the power of nobles and kings. In the United States this revolution seemed "almost to have reached its natural limits." Thus, by studying America, Tocqueville could draw lessons for the future of his own nation and all of European culture (1969, 9–12, 18).

America was so advanced in this democratic revolution, Tocqueville argued, because of several elements that conspired to produce its egalitarian point of departure. The vast stretches of land "inhabited only by wandering tribes who had not thought of exploiting" the soil enabled European immigrants to spread out and make their fortune—as opposed to nations where most lands formed parts of large hereditary estates. Settlers came chiefly from England, where they had unusual "acquaintance with notions of rights and principles of true liberty," reinforced in New England particularly by "democratic and republican" Protestant beliefs. They also came without any "idea of any superiority of some over others," because great lords did not relocate to the colonies and because the large landowners who did lacked aristocratic privileges. Instead, a "middle-class and democratic freedom" flourished almost from the outset. This combination of comparatively equal and open economic and social conditions and an ideological legacy conducive to republicanism and personal liberties made America the perfect laboratory to study the tendencies of a society that from the start was decisively free, egalitarian, and democratic in theory and practice (1969, 33–36, 50–51, 280–81).

The impact of Tocqueville's thesis on modern American scholarship was magnified by two among many works applying his ideas to twentieth-century politics, though in ways that compounded his deficiencies.[5] Each stressed one aspect of Tocqueville's account of America's point of departure. First, Gunnar Myrdal's (1944) study of American race relations emphasized the ideals of Enlightenment "humanistic liberalism." Elaborated by revolutionary leaders to define and justify their cause, these beliefs became, in Myrdal's view, the tenets of the American Creed and represented to Americans the essential meaning of their struggle for independence. It thus served as the cement of the nation, written into all the basic documents comprising the highest law of the land. This democratic creed proclaimed the worth and moral equality of all individual human beings and their "inalienable rights to freedom, justice, and a fair opportunity." It also denounced "differences made on account of 'race, creed or color' " (pp. 3–4, 7–8, 25, 52).

Since Myrdal's subject was the "Negro problem," he knew that Americans' fidelity to such beliefs was questionable. But he explained that the creed represented "valuations preserved on the general plane," which Americans knew to be *morally higher* than their discriminatory valuations. The latter were merely expressions of interests, jealousies, prejudices—impulses known to be "irrational" even by many who harbored them. Discriminations were defended, if at all, only "in terms of tradition, expediency, or utility." In Myrdal's account, then, it was this ideological inheritance, the equalitarian creed forming the national ethos, that drove American development. There was a dynamic tension between creedal values of equality and liberty, but with evident approval, Myrdal saw egalitarian values as having "triumphed" in most respects. The persistent refusal to follow American egalitarian ideals in matters of race was, he thought, most characteristic of "poor and uneducated white" people in "isolated and backward rural" areas of the deep South. Thus, his analysis offered hope that these inequalities, too, would in the end be dissolved (Myrdal 1944, lxxi–iii, 6–9; see also Jackson, 1990, 199 and Southern 1987, 295).

If Myrdal stressed Tocqueville's argument that early Americans were shaped by egalitarian Enlightenment ideals, Louis Hartz (1955) emphasized Tocqueville's account of America's relatively egalitarian and free economic and social conditions. Americans' lack of feudal institutions, classes, and their lived experience of "atomistic social freedom" made the U.S. a liberal society. Hartz viewed the presence of "the liberal idea" among early Americans as important, but he did not think it was consciousness of a specific ideological heritage that made Americans liberal. Most were instinctive—even "irrational"—Lockeans, all the more so because they had no real awareness of any alternatives. Their comparatively nonascriptive, nonhierarchical conditions led most Americans to regard liberal beliefs in individual rights and liberties, petit bourgeois democracy, and Horatio Alger myths of economic mobility as self-evident. Far more than Myrdal and even more than Tocqueville, Hartz bemoaned the fixed, dogmatic character of this liberalism born "of a liberal way of life," seeing it as a tyranny of unanimity that went much deeper than mere tyranny of the majority. He believed the absence of any real sense of class and the wide regard of middle-class values as natural supported McCarthyite antisocialist policies in domestic and foreign affairs in the early 1950s (pp. 6–23, 35–36, 46, 51, 58, 62–63, 66, 284–309).

Hartz saw conflicts in American history, but in his view they were all conflicts within liberal boundaries—between majority rule and individual or minority rights and specifically between democracy and property rights. Slavery (not true feudalism) also had to be eliminated. Yet to Hartz, these conflicts were never as deeply problematic as the stifling consensus born of lived experience from which they stemmed, "the secret root" of all that was most distinctive and fundamental about America (1955, 9, 21–22, 63, 75, 89, 91, 128–29, 140, 147).

Thus, Tocqueville, Myrdal, and Hartz differed mildly in their accounts of just why American political culture was pervasively liberal democratic and more significantly in their assessments of the desirability of that culture. But collectively, their arguments powerfully reinforced beliefs that the United States' core values should be so described. Yet all wrote at times when the nation was still denying most persons access to full standing within the American political community on racial, ethnic, or gender grounds. Their ability to stress the democratic nature of American values despite these facts is vivid testimony to how their focus on the absence of a European class system led them to minimize the significance of other types of ascriptive inequality. Each of them did, however, take some notice of America's exclusionary practices and beliefs, again in influential ways.

RACE, ETHNICITY, GENDER, AND THE TOCQUEVILLIAN THESIS

Tocqueville dealt with these issues most perceptively. Despite some misleading passages in his early chapters, he did not claim to have written an account of American political identity *in toto*. In the last chapter of volume 1 of *Democracy in America*, he said he had now finished his main task of describing democracy. But he noted that there were other things in America besides an "immense and complete democracy" that were "like tangents to my subject, being American, but not democratic." Those things were the position of two races, the "Indians and the Negroes," within (not outside) the democratic nation (1969, 316).

Thus Tocqueville distinguished being democratic from being American, though he led readers to believe that America was essentially democratic apart from these two exceptions. He also did not assume that racial conflicts would be swept away by the working out of the Revolution's egalitarian principles. He thought it more likely that the "Indian race" would resist becoming "civilized," so that it was "doomed." Tocqueville dryly underscored the inhumanity that the rhetoric of American policies toward the tribes thinly veiled; but he added that whatever those policies might be, as Europeans filled the continent, Indians would "cease to exist" (1969, 326).

Tocqueville believed that the presence of blacks was, in contrast, "the most formidable evil threatening" the nation's future, because he was not optimistic that they would ever be included in America's democracy, either. Anticipating Myrdal, Tocqueville treated racism as mere prejudice and slighted the burgeoning of scientific racist theories in Jacksonian America. But he correctly saw racism as prevalent throughout the United States even though blacks were confined to a limited area, the South. There they largely lived in slavery. But that institution was, in Tocqueville's view, uneconomic, as well as repulsive to northern Christian and Enlightenment values, so its survival was improbable. Yet should it be eliminated, Tocqueville only foresaw deepening white repugnance toward blacks. Doubting that the "white and black races will ever be brought anywhere to live on a footing of equality," especially in the United States, yet dubious of colonization efforts, he grimly concluded that a massive violent conflict between American blacks and whites was "more or less distant but inevitable" (1969, 340–63). Thus, Tocqueville did not see nonwhites as members of America's democracy, nor did he think they would become so. Instead, he expected prejudice-driven genocides.

In his first volume, devoted chiefly to political institutions, Tocqueville said nothing about women and their absence of equal political rights. That huge omission reinforced the sweeping quality of his initial descriptions of American equality. But he did consider women in what, for him, was their proper place in his second volume, primarily concerned with American civil society. There, Tocqueville presented their status as an expression of democracy's tendency to destroy or modify "those various inequalities which are in origin social," including relations such as master–servant and father–son. Tocqueville perceived a corollary tendency to make women "more nearly equal to men." He argued, however, that America was not essaying what he saw as the mistake of making men and women "creatures who are, not equal only, but actually similar." Since nature had "created such great differences between the physical and moral constitution of men and women," Americans traced "clearly distinct spheres of action for the two sexes," which both were required to keep to at all times. To attempt anything else, he stated, "degrades" both sexes (1969, 600–601).

All of this allegedly meant benefits for American women. They were viewed as competent to have major domestic responsibilities. Women were taught to think for themselves, and their husbands showed respect for their judgment. They were also not subjected to a sexual double standard: male seducers, Tocqueville claimed, were as much dishonored as their female victims. Women were also protected. Not even those in poor families had to undertake "rough laborer's work" or "hard physical exertion." But men remained the heads of families, just as they exclusively possessed voting rights and other formal political powers. Tocqueville contended that American women themselves embraced these strictures; or at least "the best of them" did, and "the others keep quiet" (1969, 590–92, 601–02).

These claims are familiar themes of "domestic sphere" ideology, though Tocqueville's statement of them provided influential reinforcement. And it is correct to say that all societies must take account of the different reproductive roles of women and men, much more clearly than they need give weight to skin color or national origins. Tocqueville's benign portrait of the condition of American women was highly romanticized, and his insistence that a social system of "separate spheres" was an appropriate response to sexual differences is no longer so widely shared. Still, nothing like a consensus exists on better answers.

Even so, Tocqueville's arguments emphatically did not establish that women were actually civic equals of men. In their different ways, women and men might have "equal worth." It might be true that American arrangements were more beneficial to women than European ones. But women were not regarded by law as rulers in their homes, were legally denied the franchise, and could not hope to occupy governmental and most professional offices. Even making public speeches on political issues was usually denied them. Thus when Tocqueville appealed to "nature" to defend all these political inequalities, as well as women's dependent status in the domestic sphere, he was endorsing a slightly modified ascriptive hierarchy that denied American women full democratic citizenship.

On close analysis, then, Tocqueville showed a rich awareness of how limited democracy was in America. But like his successors, he still frequently wrote in unqualified terms about America's supposedly egalitarian conditions; he relegated blacks and Native Americans to the status of "tangents," however important; and he obscured the intellectual respectability of racism, deeming it only prejudice. Worst of all, he claimed to reconcile the inferior civic status of women with democracy by accepting their confinement to domestic roles as natural. Hence he made America seem much more fully a liberal democracy than it was. The less comprehensive analyses of Myrdal and Hartz intensified all these failings.

Both were completely silent on women. His thorough studies of race relations, however, led Myrdal to undermine many of his opening assertions about what defines American political culture. He first made it seem that only blacks were outside the American Creed, chiefly in the south and only as a result of what most knew to be irrational biases, characteristic of the poor and uneducated. Yet as *An American Dilemma* proceeded, readers could discover that up to the very time in which Myrdal wrote, many Americans had always imputed racial inferiority to lower classes of whites and non-Anglo-Saxon immigrants, as well as blacks. Nor were those beliefs merely matters of bigoted ignorance; they were supported by the "long hegemony" of the biological sciences and medicine, "firmly entrenched" in American universities. Indeed, "scientific and popular writings with a strong racialistic bias" had "exploded in a cascade" in the years around World War I, feeding thereafter into immigration restriction. Myrdal contended that "a handful of social and biological scientists" had, in the twentieth century, gradually compelled "informed people"—but not the "ordinary man"—to give up "some of the most blatant" of racist biological beliefs (1944, 37–38, 91–92, 99, 1189, n. 10, n. 12).

Thus, far from being chiefly the prejudices of uneducated southern farmers, Myrdal showed hierarchical racial theories to have had great prestige through most of American society and history. Indeed, he eventually conceded that as political ideologies go, white supremacy should "not be denied high qualities of structural logic and consistency." And though he maintained that matters had been better in the north, he admitted that, as a result of these beliefs, "the North has kept much segregation and discrimination." Far from being chiefly an exception, moreover, Myrdal conceded that the nation's racial ordering affected virtually all aspects of American life (1944, xxiii, 97, 99, 443, 529, 599).

Myrdal did offer one answer to how academic doctrines of racial inequality squared with his claim that only liberal democratic values received "higher" intellectual defenses in America. He contended that the American Creed's very dominance calls forth dogmas of racial inequality to legitimate what are at root prejudices. And he insisted that the "philosophical" basis for such racism was the same Enlightenment outlook that had spawned liberalism. Americans favored scientific accounts of biological differences to explain their hierarchies because these accounts comported with Enlightenment attachments to rationalism (1944, 89). But those arguments hardly proved that Americans were, at bottom, philosophically liberal. If appeals to modern science are enough to show that an illiberal doctrine shares the philosophical roots of the American Creed, then Hitler's Germany and Stalin's Russia must also be held to be grounded in the bedrock Enlightenment liberalism that is supposed to make America distinctive.

Furthermore, it is not true that all major defenses of racial inequalities in the United States rested on Enlightenment rationalism. American racial justifications also drew on other traditional beliefs that were at least as intellectually influential, a point Myrdal again conceded (1944, 97). Racist readings of the Bible were immensely important. Only slight less so were doctrines of historical and cultural identity spawned by the romantics' rebellion against Enlightenment views of human nature and reason (Fredrickson 1971; Horsman 1981). If the use of religious and romantic themes to oppose egalitarianism does not count as illiberal, then writers such as Carlyle and Nietzsche may as well be placed in the liberal fold.

Louis Hartz's failure to discuss women in his two major accounts of America as a "liberal society" is more discreditable than Myrdal's, since Hartz claimed to treat America comprehensively. He did, however, address racist and nativist ideologies to some degree in *The Liberal Tradition* (1955) and more extensively in *The Founding of New Societies* (1964). Each time, he attempted to minimize their importance in ways that partly differ from Myrdal.

In his earlier work, Hartz largely ignored Native Americans, Chinese and Japanese immigrants, and other targets of racial discrimination. He dealt with race chiefly by reference to defenses for slavery in the South; and even then, he neglected theorists of racial differences. Instead, he stressed two proslavery positions that did not rely on claims of black inferiority: the states' rights constitutional arguments of John C. Calhoun and (particularly) the effort to give a neofeudal defense of the South made by George Fitzhugh.

Hartz correctly presented Fitzhugh as a "romantic nationalist," but Fitzhugh was a misleading choice to represent either proslavery views or American romanticism. Fitzhugh elaborated an "organic," paternalistic view of society that exalted slavery as beneficial for workers of any race. He opposed doctrines of racial inequality until late in his career, when he was struggling to retain an audience. Many other defenders of slavery instead embraced the scientific and religious docrines of racial hierachy (holding blacks fit only for subservient status) that pervaded antebellum America. Hartz paid little attention to their views or to the positions of the many American romantics who were antislavery Whigs. The latter were often nativists who, unlike Fitzhugh, stressed doctrines of enduring cultural differences, again hierarchically ordered. Thus by centering his discussion of blacks and race on a writer often seen as exceptional in his treatment of those issues (as Hartz admitted), Hartz illegitimately deprecated the place of overtly racialist and nativist ideologies in America (Hartz 1955, 158–72; see also Ashworth 1983, 222–23; Beer 1984; Ellis 1991, 344–51; Fredrickson 1971; Howe 1979, 234–36).

Yet not even Hartz could ignore those views entirely, and initially he conceded their illiberal character. Indeed, he argued that racist theorists like Josiah Nott forged "one of the most vicious and antiliberal doctrines of modern times," one existing "curiously enough, on a plane that was alien to liberalism and feudalism alike." But Hartz did not pause to explore this curiosity, so unaccounted for by his theory. Instead, he suggested that these alien doctrines were necessary if slaveholders were to avoid embracing feudalism for all and "keep democracy for the whites." Hartz treated this desire as evidence of their commitment to *democracy* among whites, and so it is. But it is equally evidence of southern whites' insistence on confining democracy to *whites*, while they assigned to blacks statuses that should not have existed in a liberal democratic society.

Hartz also attempted to write off these ideologies, saying they resulted only in confusion because of their conflicts with Fitzhugh's position. They were all, moreover, part of an evanescent "madhouse of Southern thought before the Civil War." Yet Hartz had to concede that a similar theory of racial supremacy, specifically Anglo-Saxon superiority, also contributed to late nineteenth-century American imperialism and Jim Crow segregation. Once more, Hartz recognized these outlooks as "basically alien to the national liberal spirit." But he asserted that they, too, had limited impact, disappearing like Josiah Nott, amounting only to "the prejudice of loose elements" amidst "the massive and uniform democratic faith" by which Americans lived (1955, 167–69, 291–92).

In light of the enduring harms they wrought on millions, Hartz's minimization of these doctrines was grotesque. The battle for civil rights in the 1960s and the scholarship that accompanied it eventually made it hard for him to dismiss American racist thought so offhandedly. In 1964, he made a different argument. American racism was really just another form of liberalism. If we "go beneath the surface of the racial attitudes," Hartz maintained, we will soon encounter what his thesis insisted must be there: the familiar figures of early liberal thinkers like "Suarez and Locke." Hartz stated that "since the European ideologies [did] not know race" and their "usual social categories" did not fit race, battles broke out within the adherents of those ideologies over their application to race. Nonetheless, actually both sides were seeking to apply the ideologies. In America's "liberal fragment" society, the only European ideology available was liberalism, so racial hierarchies had to be justified in liberal terms. The problems of doing so were massive, Hartz admitted. If blacks were human at all, liberalism demanded that they "receive full equality." He therefore claimed that Americans could oppose black equality only by consigning blacks to the status of "property" or an "inhuman species," rather than an *inferior* human species. He had to admit that after the Civil War, "the spirit of separatism continued," so that the "South won the battle of Reconstruction." Blacks were assigned just the sort of second-class status Hartz insisted that liberalism did not permit. But he claimed that the modern Civil Rights movement was proving that that status could not last (1964, 16–17, 49–50, 60–62, 102).

Hartz now also took notice of Indians, predictably stressing the (very real) influence of the Lockean argument that they had not mixed their labor with American soil enough to be able to claim it. He again treated the role of racial ideologies with near silence. And overall, Native Americans did not seem important to him. Only the fate of blacks amounted to a "major imperfection that marred the American liberal" society, having been "one of the central conscious preoccupations of our history" (1964, 94–9).

Hartz's mature answer, then, was closer to Myrdal's. Despite the contrary judgments in his earlier book, it turned out that American defenses of *racial inequality were structured in liberal terms after* all. And on liberal premises, Americans could only justify racial inequalities by denying the humanity of blacks.

This response remained wholly inadequate. It did not begin to account for why, even after constitutional recognition of the humanity of blacks, Americans created new systems of racial inequality affecting not only blacks but all nonwhite peoples and maintained them through much of the twentieth century. Hartz's appeal to recent civil rights struggles left too much history unexplained. That shortcoming reflected the deeper failure of his whole analysis: If "European ideologies" such as liberalism did not know race, where did the category of "race" come from that they had to take into account? Why had this "unknown" (and biologically indefensible) classification been a "central conscious preoccupation" throughout U.S. history? The answer is that it had been burned into American minds by prestigious intellectual traditions, most of them inarguably nonliberal, that defended subjugation of nonwhites by contending that humanity was naturally divided into hierarchically arrayed "races." There had always been much in America's basic institutions, popular sentiments, and moral orthodoxies that rendered those traditions compelling.

THE TOCQUEVILLIAN THESIS TODAY: THE ORTHODOXY ON AMERICAN IDENTITY

In an era marked by controversies over multiculturalism, one might expect the limitations of Tocqueville, Hartz, and Myrdal to have long since been superseded. But for many in the social sciences and the humanities, these Tocquevillian arguments still provide the deep structure within which they debate real but lesser differences.[6] For analyses of American politics in political science, the influence of Hartz, especially, remains pervasive. Leading realignment theorists like Walter Dean Burnham have repeatedly insisted that Hartz's theory of American political culture has "the greatest explanatory power" (Burnham 1970, 176; Burnham 1982, 15, 95, 127–28). Scholars influenced by Marxian emphases on class conflict, like Ira Katznelson, also argue that the "direction, ideological claims, and relative chances of success" of the "politics of class" in the U.S. have had their "secret root" in the liberal national character Hartz discerned (Katznelson 1981, 14–16; see also Hartz 1955, 125, 248–52). Along with many historians, a number of political scientists now see the Progressive Era as a more decisive turning point than Hartz allowed, involving basic shifts in American beliefs and institutions. But they still present the changes of that era (toward pragmatic progressivism, corporate liberalism, interest group liberalism, or some similar label) as only a mutation within the liberal institutions, ideals, and practices dominating American politics (Ackerman 1991; Ceaser 1979; Galambos 1970, 1983; Hays 1957; Kloppenberg 1986; Lowi 1979; Lustig 1982; Sklar 1991; Wiebe 1967).

Historically minded students of American culture have often found Hartz too simplistic; but they have usually sought to add complexities without disputing the basic Tocquevillian framework. The one major effort to dethrone Hartz (but not Tocqueville) has been the "republican synthesis" identified with Bernard Bailyn (1967), Gordon Wood (1969), and J. G. A. Pocock (1975). But even proponents of republican historiography now concede that Americans have more often blended liberalism and republicanism than opposed them and that at some point in U.S. history liberalism became predominant (Ackerman 1991, 27–29, 327, n. 45). Many scholars (e.g., Sacvan Bercovitch and John Diggins) have also argued that Hartzian accounts underplay the role of Protestant values; but most present these religious strains as reaffirming the American liberal consensus despite serving as sources of criticism (Bercovitch 1978; Diggins 1984; Greenstone 1986). Hence, these writings pose significant debates within—but not challenges to—interpretations explaining American evolution in terms of liberal republican preconditions.

None of these mainstream approaches to American politics has given prominence to the racial, ethnic, or gender makeup of the American citizenry, though neither have they wholly avoided those issues. In the last three decades, however, many other scholars have greatly enriched understanding of the ethnocultural dimensions of American life. Much of this research provides evidence for a multiple-traditions account of American politics. But few of these scholars have addressed the significance of their findings for general views of America. And, perhaps because of the real if partial truths grasped by the Tocquevillian orthodoxy, those who have done so have usually tried to accommodate it, not to challenge it.

Most important in this regard is the seminal study of nativist and racist ideologies in modern American scholarship, John Higham's *Strangers in the Land,* first published in 1955. Higham's book had many ingredients needed to correct the Tocquevillian thesis. He correctly saw American nativism as a species of modern nationalism. He also believed it was built on ethnocentric attitudes that were virtually always present. But Higham stressed that only in certain periods did American leaders elaborate those attitudes into full-fledged ideologies (1966, 4). (Thus, he actually found several nativisms, though all shared an "ideological core.")

This distinction between an elaborated ideology and more inarticulate ethnocentric feelings has some force, though Higham has acknowledged that it also presents difficulties (1986, 223; 1988, 343–44). But one consequence was that for most readers Higham's work did not compel any major reinterpretation of American politics. Nativist ideologies could still be seen as occasional things, fitting the pattern of exceptions to egalitarian beliefs that Tocquevillian scholars continued to stitch. In contrast, liberal democratic ideology still seemed more constant and intellectually developed, as Myrdal had argued.

That contrast is not defensible. As Myrdal had to admit, sophisticated doctrines of racial inequality were dominant in American universities and public opinion through much of U.S. history. And as Hartz recognized, Americans ordinarily have not held liberal democratic values in the form of full-fledged ideologies any more than they have racial values. Liberal democratic norms have often been unreflective if not irrational sets of beliefs, just as deserving of the label "prejudices" as racial values. It is, then, not credible to distinguish nativistic Americanism from liberalism and republicanism on the ground that Americanism has usually been a set of ethnocentric attitudes while the latter have been articulated ideologies. Insofar as Higham's presentation of nativist ideology as an occasional thing has permitted many to believe that America has otherwise been liberal democratic, it has been misleading.

Even so, Higham's work, along with the Civil Rights movement, has justly inspired many scholars to analyze American attitudes and practices involving blacks, women, Native Americans, Chinese, Japanese, Jews, Mexicans, and the whole panoply of groups living in the United States. Perhaps if Higham had explicitly considered whether nativist ideologies really fit into Tocquevillian accounts of America, more writers might have addressed that issue. But as Martin Sklar has observed, most scholars today "essentially tell their stories within the framework" of traditional accounts of American political culture "or some mixture of them" (1991, 79). Many do so simply by focusing on the experience of one "outsider" group without exploring whether that group's experience raises questions about portraits of American political culture as otherwise inclusive and egalitarian (e.g., Bell 1987; Fredrickson 1971; Miller 1969). There are, however, several ways in which many contributors to the new scholarship of race, ethnicity, and gender explicitly preserve the traditonal framework.

Some accounts employ versions of neo-Marxian—usually Gramscian—premises stressing the presence of an economically rooted liberal hegemony in America and treating racist and sexist ideologies as appended rationales for forms of economic exploitation that blatantly violate liberal democratic precepts (Fields 1990; Takaki 1979). These scholars are right to see ideologies of racial and sexual inequality as social constructions justifying systems of unequal power and status, and the people privileged by those systems have also generally held lofty economic statuses. The powerful tend to seek superiority in every social arena. But those facts do not render liberal democracy the basic American ideology and racial or sexual ideologies merely "an inconsistent afterthought," designed to explain the anomalous status of a minority of the population (Fields 1990, 114, 117). On this view, after all, these ideologies are all myths justifying economic exploitation. And even though economic supremacy is vital to those who enjoy it, it is easy to see why belonging to a caste proclaimed intrinsically superior might have charms beyond its economic benefits. Why, then, should the ideologies and institutions of racial and gender hierarchies be deemed "afterthoughts," instead of key components of American political culture? Again, American law has denied rights of full citizenship much more often on these ascriptive grounds than because of class (though class has mattered greatly).

Scholars also attempt to link racial and sexual inequalities with the concept of the United States as a liberal society by drawing on postmodernist arguments. They hold that the discourses and practices dominant in Enlightenment liberal societies recurringly construct the identities of marginal groups as irrational, passionate, dangerous "others," both to defend their exploitation and to deny the presence of such qualities in mainstream citizens (Karst 1989; Norton 1986; Rogin 1975). Even if we grant power to these contentions, they do not justify claims like Michael Rogin's that "liberal egalitarian" values are primary in America but have a "logical marriage" to racist exploitation (Rogin 1975, 279). This argument might be compelling if the United States really had begun as an essentially liberal democratic society and had then generated racial and sexual inequalities out of that society's tensions. But colonial British Americans pursued practices of racial and gender domination long before they embraced the types of liberal republican ideologies and institutions that came to play prominent roles in America. Hence once more it is plausible to see all these different practices and ideologies as central components in American political development, rather than some as primary and others as secondary. Their recurring admixture also does not prove that ascriptive inegalitarian outlooks have been *logically* compatible with liberal democracy, because people often operate for long periods while holding contradictory beliefs.

Feminist scholars have been especially concerned to work out the relationship of liberalism to doctrines of sexual inequality. But far from criticizing Tocquevillian descriptions, most have striven to confirm that modern forms of patriarchy derive from liberal ideas. Thus, Carole Pateman objects when other feminists treat liberal capitalism and patriarchy as two systems that are "intertwined" but "relatively autonomous." She insists that liberal thought has always had a patriarchal structure that is essential to it. She agrees with Tocqueville that the subordinate status of American women has been not only consistent with, but expressive of, liberal democratic principles.

Yet Pateman acknowledges that the premise of classical liberal contract theory—that all people are "naturally free and equal"—is potentially "subversive of all authority relations, including conjugal relations." She contends, correctly, that early liberal theorists like Locke responded by asserting women were not naturally equal to men. But she also observes, correctly, that these writers were "extremely vague" on what capacities relevant to moral and political equality women lacked. Indeed, they sometimes conceded that women had sufficient capacities to enter contracts as equals (Pateman 1988, 38, 41, 54, 94). Pateman thus provides undeniable evidence that liberal writers endorsed conventional beliefs in natural sexual inequality; but far from showing that their liberal precepts required or generated those beliefs, her citations indicate that theorists like Locke did not really reconcile their inherited patriarchal beliefs with their more novel, distinctively liberal arguments. Hence, it seems quite reasonable to view liberal precepts and patriarchy as two intertwined but relatively autonomous systems of ideas and practices that contract theorists and many Americans have often inconsistently endorsed (see Eisenstein 1981, 3–5, 34–49; Mackinnon 1987, 14–16, 164–65; Okin 1979, 199).

Some writers (especially lawyers associated with Critical Race Studies) have criticized how other leftist scholars treat all American institutions as expressive of liberalism, instead of recognizing racism as an often contrasting and "central ideological underpinning of American society" (Crenshaw 1988, 1336). These arguments move in the direction pursued here, but they are still rare and do not yet extend to explicit critiques of Tocquevillian frameworks or to any developed alternatives. Despite the new scholarship of race, ethnicity, and gender, then, liberalism is still usually understood to have been both the chief ideology of opposition to racism, nativism, and patriarchy in America and the chief (or even sole) ideology supporting them.

As a result, all recent major works addressing the general topics of American nationality and citizenship endorse some version of this paradoxical Hartzian position. Many quote Philip Gleason's summation of what has historically been required to be fully

American: "A person did not have to be of any particular national, linguistic, religious, or ethnic background. All he had to do was commit himself to the political ideology centered on the abstract ideals of liberty, equality, and republicanism. Thus the universalist ideological character of American nationality meant that it was open to anyone who willed to become an American" (1980, 62). Gleason adds that "universalism had its limits from the beginning, because it did not include either blacks or Indians, and in time other racial and cultural groups were regarded as falling outside the range of American nationality." There was "a latent predisposition toward an ethnically defined concept of nationality." But this "exclusiveness ran contrary to the logic of the defining principles, and the official commitment to those principles has worked historically to overcome exclusions and to make the practical boundaries of American identity more congruent with its theoretical universalism" (pp. 62–63). Despite the nation's record of blatant, not latent, exclusions, then, Gleason still suggests that the basic official requirement for full membership in the American political community has always been willingness to embrace liberal republican principles and that those principles have inexorably delegitimated all inconsistent exclusions.[7]

As the 1990s began, two noted works reexamined America's "civic culture" with explicit attention to race, ethnicity, and gender without truly modifying these Tocquevillian claims: Kenneth Karst's (1989) *Belonging to America* and Lawrence Fuchs's (1990) *American Kaleidoscope*.[8] Karst's book is historically richly informed but focuses on recent legal developments, for his main aim is to lay out the constitutional implications of "the egalitarian strand in our civic culture." Karst asks readers whether these egalitarian values "ring true as part of what many Americans today accept as our national tradition," without insisting that most Americans always did so (1989, 33, 42, 217). Even so, he cites Gleason and concurs that the American national ideology has led to the gradual "enlargement of the national community." But Karst knows well that American policies have long voiced precepts—including "Protestant domination, white supremacy, and the dependency of women on men"—that are diametrically opposed to what he terms the "central values" of "today's American civic culture." He therefore takes a step Myrdal and Hartz resisted. Karst states that the American Creed has had "self-contradictory" elements, and he holds that Americans have been guilty of "hypocrisy," producing "huge exceptions" to our willingness to live up to our egalitarian ideals (pp. 30–32, 40, 47, 62, 172, 179, 181, 188, 210–11, 215, 228, 242).

These accusations of inconsistency and hypocrisy are significant concessions to the prevalence of inegalitarian traditions; but they still presume that Americans' more liberal and democratic beliefs are their "real" ones. Karst does not explain why, given the contradictory elements of our ideology, only conformity with the egalitarian ones counts as consistency and only compliance with the creed's nonliberal values constitutes hypocrisy. Nor does he clarify why the exclusionary policies that have prevailed during most of U.S. history should be identified as "exceptions" (however "huge") to its ideals. By failing even to raise these issues, Karst largely leaves the conventional narrative of American membership intact.

Lawrence Fuchs's massive study of ethnicity and race in America defines the nation's *civic culture* as based essentially on three beliefs derived from the founders' understanding of republicanism: that "ordinary men and women" are entitled to representative self-governance, that "all who live in the political community" should be able to "participate in public life as equals," and that citizens should have freedom for different religious outlooks and other sorts of pursuits in their private lives (1990, 4–6). Fuchs structures his book around (1) a discussion of this civic culture, characterized by the "voluntary pluralism" these principles support, and (2) three surveys of those long kept "outside the civic culture" by coercion: Native Americans; African–Americans; Asians and Mexicans. Next, Fuchs tells the story of the "triumph of the civic culture," the movement of these peoples toward full inclusion.

His book is, in its details, quite free of factual illusions. Fuchs notes that "the Euro–American determination to maintain a racially exclusive civic culture" was not abandoned until the 1960s to 1980s, making clear that such efforts have been powerful through the bulk of U.S. history (1990, 79). Yet in three ways, the design of Fuchs's analysis gives unwarranted support to whiggish narratives of American progress toward full conformity with the nation's inclusive "core" principles. First and most obviously, Fuchs (like his teacher Hartz) does not discuss the exclusion of women from full citizenship. It is true that women were always said to be American citizens, while many members of racial minorities were not; and it is hard for one book to cover all topics. But Fuchs contends that the civic culture fostered by the founders trusts *both* men and women to elect representatives and that all in the political community should be able to participate in public life as equals. Since women did not so participate through most of U.S. history, Fuchs must add that the founders extended this principle only to "white adult males." There is then no reason why an account of those outside his civic culture should not include the struggles of women to participate in public life as equals. Such omissions or minimizations of excluded groups characterize every author in the Tocquevillian tradition. Although many writers have had justifications for their emphases, the cumulative effect of these persistent failures to lay out the full pattern of civic exclusions has been to make it all too easy for scholars to conclude that egalitarian inclusiveness has been the norm. Once we recall that the exceptions have often defined the status of over half the domestic adult population in the United States (a fact that forces us to take note of the unequal civic status of women), then the exceptions obviously have great claim to be ranked as rival norms.

Second, Fuchs's definition of his organizing category, the civic culture, is questionable. Why should only the principles encompassed by "voluntary pluralism" count as parts of the civic culture? Why should the principles officially employed to deny eligibility for full citizenship to so many others be treated as "outside" that culture? It is surely more appropriate to treat the nation's political culture as including all American residents, and the rationales for denying political equality to many. Fuchs's narrow use of "civic culture" provides illicit support for his claim that the "core of the national community" has been voluntaristic liberal values, despite the masses subjected to "coercive pluralisms."

Finally, Fuchs concludes that by the 1980s, the struggles against the various "old systems" of ascriptive inequality "had succeeded" and that problems of "racial, religious, a nationality conflict" were no longer so serious. *Class* barriers to social progress had now become the most difficult to overcome (1990, 492–93). Perhaps so. But Fuchs has structured his whole historical analysis in ways that minimize ethnic and racial ideologies, and he never explores why such illiberal ideas and practices have had recurring power in American life. Thus his optimism that they have been laid to rest may not be justified. The contrasting lessons suggested by a multiple-traditions approach to American politics can be seen by analyzing the development of American laws of nationality and citizenship following Reconstruction—a period in many ways parallel to our own.

THE MULTIPLE-TRADITIONS THESIS OF AMERICAN CIVIC IDENTITY

It seems prudent to stress what is not proposed here. This is not a call for analysts to minimize the significance of white male political actors or their conflicts with each other. Neither is it a call for accounts that assail "Eurocentric" white male oppressors on behalf of diverse but always heroic subjugated groups. The multiple-traditions thesis holds that Americans share a *common* culture but one more complexly and multiply constituted than is usually acknowledged. Most members of all groups have shared and often helped to shape all the ideologies and institutions that have structured American life, including ascriptive ones. A few have done so while resisting all subjugating practices. But members of every group have sometimes embraced "essentialist" ideologies valorizing their own ascriptive traits and denigrating those of others, to bleak effect. Cherokees enslaved blacks, (Perdue, 1979), champions of women's rights disparaged blacks and immigrants, (DuBois 1978); and blacks have often been hostile toward Hispanics and other new immigrants (Daniels 1990, 323, 376). White men, in turn, have been prominent among those combating invidious exclusions, as well as those imposing them.

Above all, recognition of the strong attractions of restrictive Americanist ideas does not imply any denial that America's liberal and democratic traditions have had great normative and political potency, even if they have not been so hegemonic as some claim.[9] Instead, it sheds a new—and, in some respects, more flattering—light on the constitutive role of liberal democratic values in American life. Although some Americans have been willing to repudiate notions of democracy and universal rights, most have not; and though many have tried to blend those commitments with exclusionary ascriptive views, the illogic of these mixes has repeatedly proven a major resource for successful reformers. But we obscure the difficulty of those reforms (and thereby diminish their significance) if we slight the ideological and political appeal of contrary ascriptive traditions by portraying them as merely the shadowy side of a hegemonic liberal republicanism.

At its heart, the multiple-traditions thesis holds that the definitive feature of American political culture has been not its liberal, republican, or "ascriptive Americanist" elements but, rather, this more complex pattern of apparently inconsistent combinations of the traditions, accompanied by recurring conflicts. Because standard accounts neglect this pattern, they do not explore how and why Americans have tried to uphold aspects of all three of these heterogeneous traditions in combinations that are longer on political and psychological appeal than on intellectual coherency.

A focus on these questions generates an understanding of American politics that differs from Tocquevillian ones in four major respects. First, on this view, purely liberal and republican conceptions of civic identity are seen as frequently unsatisfying to many Americans, because they contain elements that threaten, rather than affirm, sincere, reputable beliefs in the propriety of the privileged positions that whites, Christianity, Anglo-Saxon traditions, and patriarchy have had in the United States. At the same time, even Americans deeply attached to those inegalitarian arrangements have also had liberal democratic values. Second, it has therefore been typical, not aberrational, for Americans to embody strikingly opposed beliefs in their institutions, such as doctrines that blacks should and should not be full and equal citizens. But though American efforts to blend aspects of opposing views have often been remarkably stable, the resulting tensions have still been important sources of change. Third, when older types of ascriptive inequality, such as slavery, have been rejected as unduly illiberal, it has been normal, not anomalous, for many Americans to embrace new doctrines and institutions that reinvigorate the hierarchies they esteem in modified form. Changes toward greater inequality and exclusion, as well as toward greater equality and inclusiveness, thus can and do occur. Finally, the dynamics of American development cannot simply be seen as a rising tide of liberalizing forces progressively submerging contrary beliefs and practices. The national course has been more serpentine. The economic, political, and moral forces propelling the United States toward liberal democracy have often been heeded by American leaders, especially since World War II. But the currents pulling toward fuller expression of alleged natural and cultural inequalities have also always won victories. In some eras they have predominated, appearing to define not only the path of safety but that of progress. In all eras, including our own, many Americans have combined their allegiance to liberal democracy with beliefs that the presence of certain groups favored by history, nature, and God has made Americans an intrinsically "special" people. Their adherents have usually regarded such beliefs as benign and intellectually well founded; yet they also have always had more or less harsh discriminatory corollaries.

To test these multiple-traditions claims, consider the United States in 1870. By then the Civil War and Reconstruction had produced dramatic advances in the liberal and democratic character of America's laws. Slavery was abolished. All persons born in the United States and subject to its jurisdiction were deemed citizens of the United States and the states in which they resided, regardless of their race, creed or gender. None could be denied voting rights on racial grounds. The civil rights of all were newly protected through an array of national statutes. The 1790 ban on naturalizing Africans had been repealed, and expatriation declared a natural right. Over the past two decades women had become more politically engaged and had begun to gain respect as political actors.

Confronted with these developments, what would Tocquevillian analysts have predicted for the next half-century of American life? Louis Hartz would have insisted that so long as the humanity of blacks, other races, and women was publicly acknowledged, the United States would have to grant them equal access to full citizenship. Myrdal, Karst, and Fuchs would have anticipated that surviving prejudices might produce resistance to implementation of the new legal expressions of the American Creed; but they would expect this opposition to be gradually, if painfully, overcome. Tocqueville on the other hand, would have been too pessimistic. He would have deplored the intrusion of women into politics, expected Native Americans to continue toward extinction, and foreseen deepening conflicts between whites and blacks that would probably end in some sort of destructive cataclysm.

None would have had the intellectual resources to explain what in fact occurred. Over the next fifty years, Americans did not make blacks, women, and members of other races full and equal citizens, nor did racial and gender prejudices undergo major erosion. Neither, however, were minorities and women declared to be subhuman and outside the body politic. And although white Americans engaged in extensive violence against blacks and Native Americans, those groups grew in population, and no cataclysm loomed. Instead, intellectual and political elites worked out the most elaborate theories of racial and gender hierarchy in U.S. history and partially embodied them in a staggering array of new laws governing naturalization, immigration, deportation, voting rights, electoral institutions, judicial procedures, and economic rights—but only partially. The laws retained important liberal and democratic features, and some were strengthened. They had enough purchase on the moral and material interests of most Americans to compel advocates of inequality to adopt contrived, often clumsy means to achieve their ends.

The considerable success of the proponents of inegalitarian ideas reflects the power these traditions have long had in America.[10] But after the Civil War, Spencerian and Darwinian evolutionary theories enormously strengthened the intellectual prestige of doctrines presenting the races and sexes as naturally arrayed into what historians have termed a "raciocultural hierarchy," as well as a "hierarchy of sex." Until the end of the nineteenth century, most evolutionists were neo-Lamarckians who thought acquired characteristics could be inherited. Thus beliefs in biological differences were easily merged with the Teutonist historians' views that peoples were the products of historical and cultural forces. Both outlooks usually presented the current traits of the races as fixed for the foreseeable future. Few intellectuals were shy about noting the implications of these views for public policy. Anthropologist Daniel G. Brinton made typical arguments in his 1895 presidential address to the American Association for the Advancement of Science. He contended that the "black, brown and red races" each had "a peculiar mental temperament which has become hereditary," leaving them constitutionally "recreant to the codes of civilization." Brinton believed that this fact had not been adequately appreciated by American lawmakers. Henceforth, conceptions of "race, nations, tribes" had to "supply the only sure foundations for legislation; not *a priori* notions of the rights of man" (1895, p. 249; see also Boller 1969, 180–85; Degler 1991, 15–16, 107, 397; Haller 1971, 11, 125–27; Ross 1991, 64–77; Russett 1989, 74–75, 204–5; Stocking 1968, 55, 122).

As Brinton knew, many politicians and judges had already begun to seize on such suggestions. In 1882, for example, California senator John Miller drew on the Darwinian "law of the 'survival of the fittest' " to explain that "forty centuries of Chinese life" had "ground into" the Chinese race characteristics that made them unbeatable competitors against the free white man. They were "automatic engines of flesh and blood," of "obtuse nerve," marked by degradation and demoralization, and thus far below the Anglo-Saxon, but were still a threat to the latter's livelihood in a market economy. Hence, Miller argued, the immigration of Chinese laborers must be banned. His bill prevailed, many expressing concern that these Chinese would otherwise become American citizens (Miller 1882, 1484–85, cf. 1548, 1583). The Chinese Exclusion Act was not a vestige of the past but something new, the first repudiation of America's long history of open immigration; and it was justified in terms of the postwar era's revivified racial theories.

Yet although men like Miller not only sustained but expanded Chinese exclusions until they were made virtually total in 1917 (and tight restrictions survived until 1965), they never managed to deny American citizenship to all of the "Chinese race." Until 1917 there were no restrictions on the immigration of upper-class Chinese, and in 1898 the Supreme Court declared that children born on U.S. soil to Chinese parents were American citizens (Daniels 1990, 278; *United States* v. *Wong Kim Ark* 1898). Birthplace citizenship was a doctrine enshrined in common law, reinforced by the Fourteenth Amendment, and vital to citizenship for the children of *all* immigrant aliens. Hence it had enough legal and political support to override the Court's recognition of Congress's exclusionary desires. Even so, in other cases the Court sustained bans on Chinese immigration while admitting the racial animosities behind them, as in the "Chinese Exclusion Case" (*Chae Chan Ping* v. *United States* 1889); upheld requirements for Chinese–Americans to have certificates of citizenship not required of whites (*Fong Yue Ting* v. *United States* 1893); and permitted officials to deport even Chinese persons who had later been judged by courts to be native-born U.S. citizens (*United States* v. *Ju Toy* 1905).

The upshot, then, was the sort of none-too-coherent mix that the multiple-traditions thesis holds likely. Chinese were excluded on racial grounds, but race did not bar citizenship to those born in the United States; yet Chinese ancestry could subject some American citizens to burdens, including deportation, that others did not face. The mix was not perfect from any ideological viewpoint, but it was politically popular. It maintained a valued inclusive feature of American law (birthplace citizenship) while sharply reducing the resident Chinese population (Daniels 1990, 240). And it most fully satisfied the increasingly powerful champions of Anglo–Saxon supremacy.

From 1887 on, academic reformers and politicians sought to restrict immigration more generally by a means that paid lip service to liberal norms even as it aimed at racist results—the literacy test. On its face, this measure expressed concern only for the intellectual merits of immigrants. But the test's true aims were spelled out in 1896 by its sponsor, Senator Henry Cabot Lodge, a Harvard Ph.D. in history and politics. Committee research, he reported, showed that the test would exclude "the Italians, Russians, Poles, Hungarians, Greeks, and Asiatics," thereby preserving "the quality of our race and citizenship." Citing "modern history" and "modern science," Thomas Carlyle and Gustave le Bon, Lodge contended that the need for racial exclusion arose from "something deeper and more fundamental than anything which concerns the intellect." Race was above all constituted by moral characteristics, the "stock of ideas, traditions, sentiments, modes of thought" that a people possessed as an "accumulation of centuries of toil and conflict." These mental and moral qualities constituted the "soul of a race," an inheritance in which its members "blindly believe," and upon which learning had no effect. But these qualities could be degraded if "a lower race mixes with a higher"; thus, exclusion by race, not reading ability, was the nation's proper goal (Lodge 1896, 2817–20).

When the literacy test finally passed in 1917 but proved ineffective in keeping out "lower races," Congress moved to versions of an explicitly racist national-origins quota system. It banned virtually all Asians and permitted European immigration only in ratios preserving the northern European cast of the American citizenry. Congressman Albert Johnson, chief author of the most important quota act in 1924, proclaimed that through it, "the day of indiscriminate acceptance of all races, has definitely ended." The quota system, repealed only in 1965, was a novel, elaborate monument to ideologies holding that access to American citizenship should be subject to racial and ethnic limits. It also served as the prime model for similar systems in Europe and Latin America (Daniels 1990, 282–84; Dowty 1987, 90–91).

Lodge, the architect of racist immigration restrictions, was a Republican who in 1890 had barely failed to push through a bill reviving enforcement of Reconstruction civil rights statutes. In addition to partisan motives, that effort had reflected Yankee beliefs that northern culture could lift up American blacks. Soon, however, even Boston Brahmins like Lodge, along with religious leaders and scholars in almost every field, began admitting that their racial ideologies undercut the case for equal rights for all American people of color. Compulsion to do so came not only from their advocacy of immigration restrictions but also from their support for America's new imperialism. The nation's colonial acquisitions in the Pacific and the Caribbean during the late 1890s permitted its leaders to feel more comparable to the great powers of Europe, who shared the "White Man's Burden." And most leaders accepted, as the *Nation* wrote in 1898, that the "varied assortment of inferior races" inhabiting America's new possessions "of course, could not be allowed to vote." Eventually, in 1917, Puerto Ricans were made U.S. citizens but, again, of a lower grade. They were not granted votes in federal elections, nor did they receive all the constitutional rights of other citizens. The Supreme Court sustained these positions, holding that while Puerto Ricans were now citizens, Puerto Rico had not truly been "incorporated" into the United States (*Balzac* v. *Puerto Rico* 1922; Higham 1966, 108–9; Painter 1987, 142, 147, 152, 161; Woodward 1966, 72–74). Thus here, too, Americans constructed a civic status that did not fully satisfy either those who believed that all U.S. citizens should have equal rights or those who thought that inferior races should be denied citizenship. It was another of the "anomalous" statuses that somehow encompassed the majority of all Americans.

What Myrdal rightly termed the "explosion" of racist ideologies during these years also abetted the most famous example of such a mixed status—the Jim Crow system of "separate but equal" laws and practices (Woodward 1966, 74). Sobering evidence in this regard is a speech by Charles Francis Adams, Jr. (1908) to a Virginia audience. Adams was the great grandson of a signer of the Declaration of Independence, grandson of an antislavery congressman, and a veteran of both the Union Army and liberal reform movements. But he now said that the "'glittering generalities' of the Declaration of Independence" and the beliefs in racial equality during Reconstruction seemed "strangely remote, archaic even." The scientific views of Darwin had superseded scriptural views of the brotherhood of man, making it clear that the Reconstruction policy of 1866 was a blunder that was "worse than a crime." The solution to the "race problem" now had to be "worked out in the South," without northern protests against segregation (pp. 16–19).

The renewed acceptance of doctrines of racial hierarchy after 1870 is also visible in judicial rulings like that of Oregon district judge Matthew Deady, a rare champion of Chinese immigrant rights. In 1880 he ruled that a person of half Native American descent could not be naturalized because he was neither white nor of African ancestry, as the 1870 naturalization statute required. Deady wrote that it might seem strange that blacks could gain citizenship but that "the intermediate and much-better-qualified red and yellow races" could not. He explained that Africans were "not likely to emigrate," so that the 1870 act was "merely a harmless piece of legislative buncombe" (*In re Camille* 1880). Such disdain for Reconstruction laws hardly communicated to southern whites that those measures had to be strictly observed.

But despite the new prevalence of such attitudes on the part of northern and western elites in the late nineteenth century, the Reconstruction amendments and statutes were still on the books, and surviving liberal sentiments made repealing them politically difficult. Believers in racial inequality were, moreover, undecided on just what to do about blacks. As Joel Williamson has shown, "Radical" racists (e.g., the nation's chief statistician, Cornell professor Walter Willcox) argued that blacks, like other lower races, should be excluded from American society and looked hopefully for evidence that they were dying out. Their position was consistent with Hartz's claim that Americans could not tolerate permanent unequal statuses; persons must either be equal citizens or outsiders. But those whom Williamson terms "Conservatives" believed, like the antebellum Whigs, that blacks and other people of color might instead have a permanent "place" in America, so long as "placeness included hierarchy." Some still thought that blacks, like the other "lower races," might one day be led by whites to fully civilized status, but no one expected progress in the near future. Thus blacks should instead be segregated, largely disfranchised, and confined to menial occupations via inferior education and discriminatory hiring practices—but not expelled, tortured, or killed. A few talented blacks might even be allowed somewhat higher stations (Williamson 1984, 7, 28–29, 86, 122, 224; see also Ashworth 1983, 222–23).

In the heyday of Jim Crow, Radical racist views were most influential, far stronger than fading liberal ones, but they were not powerful enough to generate the violent elimination of blacks that Tocqueville feared and many white racists sought. Instead, the result was a system closest to Conservative desires, one that kept blacks in their place, although that place was structured more repressively than most Conservatives favored. And unlike the ineffective literacy test, here racial inegalitarians achieved much of what they wanted without explicitly violating liberal legal requirements. Complex registration systems, poll taxes, and civics tests appeared race-neutral but were designed and administered to disfranchise blacks. This intent was little masked. Even progressives like Carter Glass called openly for achieving racial disfranchisement by indirect means. He urged the 1901–2 Virginia Constitutional Convention to adopt every "discrimination within the letter of the law" that promised "the elimination of every Negro voter who can be gotten rid of." These efforts succeeded. Most dramatically, in Louisiana 95.6% of blacks were registered in 1896, and over half (130,000) voted. After disfranchising measures, black registration dropped by 90% and by 1904 totaled only 1,342. The Supreme Court found convoluted ways to close its eyes to these tactics (Kousser 1974, 49, 262; Schmidt 1982, 846–47; Woodward 1966, 90–92; and see, e.g., *Giles* v. *Harris* 1903 and *Giles* v. *Teasley* 1904).

By similar devices, blacks were virtually eliminated from juries in the south, where 90% of American blacks lived, sharply limiting their ability to have their personal and economic rights protected by the courts (Schmidt 1983). "Separate but equal" educational and business laws and practices also stifled the capacities of blacks to participate in the nation's economy as equals, severely curtailed the occupations they could train for, and marked them—unofficially but clearly—as an inferior caste. Thus here, as elsewhere, it was evident that the nation's laws and institutions were not meant to confer the equal civic status they proclaimed for all Americans; but neither did they conform fully to doctrines favoring overt racial hierarchy. They represented another asymmetrical compromise among the multiple ideologies vying to define American political culture.

So, too, did the policies governing two groups whose civic status formally improved during these years: Native Americans and women. In 1884, the Supreme Court ruled that Native Americans were not native-born U.S. citizens, even though they were born on lands over which the United States claimed sovereignty (*Elk* v. *Wilkins* 1884). They were "wards" inhabiting "domestic dependent nations." Yet federal policy in these years aimed at making them U.S. citizens, and eventually all were made so by the Indian Citizenship Act of 1924. To U.S. officials, preparing Native Americans for citizenship meant "civilizing" them, that is, displacing their traditional religions, family structures, and systems of subsistence, landholding, and tribal governance with Christianity, heterosexual monogamy, and self-sufficient farming on individually held lands, thereby ending tribal existences. The 1887 Dawes General Allotment Act was a keystone of this effort. It reassigned tribal lands and extended U.S. citizenship to individual Native Americans, subject to a 25-year period of federal trusteeship before those individuals could assume full land rights (and hence full rights as citizens). Not incidentally, the act also made huge amounts of "surplus" tribal lands available to whites. These assimilationist policies were defended by the same racial theories that argued for tutelary Anglo-Saxon governance of "lower races" abroad. Indeed, many legislators not only supported but linked Native American assimilation and imperialist policies. And even after 1924, states subjected their new Native American citizens to disfranchising devices and other forms of discrimination similar to those imposed on blacks. Hence although Native Americans became citizens in this era, the process was more coercive than consensual, and they, too, became Americans who were sometimes treated as bearers of equal rights, sometimes as a group subject to restrictions that other Americans did not face (Ragsdale 1989, 406–15; Williams 1980, 823–28; Wolfley 1991, 177–89).

Women took a major step toward full citizenship when they gained the vote via the Nineteenth Amendment in 1920. But women were little more favored than nonwhites by the evolutionary theorists of the late nineteenth and early twentieth centuries. Darwin wrote that many of the defining characteristics of women were "characteristics of the lower races, therefore of a past and lower state of civilization," typified by the domination of sexual and spiritual emotions over reason. Thus women, like black men, were often lovable but ultimately childlike (Degler 1991, 26; Russett 1989, 11–12). Many female activists did not repudiate these views so much as adapt them, allowing women's special vulnerabilities to justify laws "protecting" them in the workplace and contending that their domestic nature gave women insights into social problems that would render them valuable in the voting booth (Cott 1987, 25–27, 53–81; Flexner 1975, 274–323).

Thus when American women gained the suffrage, for many the struggle for gender equality had not culminated but rather had been limited, postponed, or abandoned. Until 1922, federal measures taken during this era deepened, rather than modified, the dependence of women's citizenship on that of their husbands (see, e.g., *Mackenzie* v. *Hare* 1915). Although the 1922 Cable Act permitted women to retain American citizenship even if they married aliens, it excepted those who married men "ineligible by race for naturalization." And long after 1920, legislatures and courts maintained rules that discouraged female voting and jury service (Smith 1989, 273–84). Despite some major modifications, then, women, like blacks, Native Americans, and most Asian Americans, remained both officially equal citizens and legitimately subject to special restrictions based on their perceived biological and cultural weaknesses.

This period also highlights how the influence of inegalitarian doctrines has not been confined to white male intellectuals, legislators, and judges. The leading writer of the early twentieth-century women's movement, Charlotte Perkins Gilman, was a thoroughgoing Darwinian who accepted that evolution had made women inferior to men in certain respects, although she insisted that these differences were usually exaggerated and that altered social conditions could transform them (Gilman 1966, 99–145; Russett 1989, 13–14). And even as he attacked Booker T. Washington for appearing to accept the "alleged inferiority of the Negro race," W. E. B. DuBois embraced the widespread Lamarckian view that racial characteristics were socially conditioned but then inherited as the "soul" of a race. He could thus accept that most blacks were "primitive folk" in need of tutelage, of an "emotional," mystical, "essentially artistic" nature. Hence they were best led by the "Talented Tenth" who had risen, like DuBois himself, to civilized rationality (Reed 1992, 131–37).

The acceptance of ascriptive inegalitarian beliefs by brilliant and politically dissident female and black male intellectuals strongly suggests that these ideas had broad appeal. Writers whose interests they did not easily serve still saw them as persuasive in light of contemporary scientific theories and empirical evidence of massive inequalities. It is likely, too, that for many the vision of a meaningful natural order that these doctrines provided had the psychological and philosophical appeal that such positions have always had for human beings, grounding their status and significance in something greater and more enduring than their own lives. No doubt that grounding was especially reassuring to many old-stock Americans whose material, political, and social positions had been unsettled by immigration, industrialization, and urbanization, as many argue. But these inegalitarian views should not be seen as simply reactive and fearful. For many, they spawned confidence that the intellectual keys to a better future had been found. And these views were often widely shared by constituencies that consisted of much more than simply capitalists, or the Protestant middle class, or even that group combined with native-born workers. In an 1879 referendum, for example, Californians cast 154,638 ballots against permitting Chinese immigration, 883 in favor; Nevada had similar results the next year (Hutchinson 1981, 75–77). Such overwhelming support suggests that these doctrines appealed to Americans' anxieties and economic interests, but also to their loyalties, their hopes, and their minds.

In sum, if we accept that ideologies and institutions of ascriptive hierarchy have shaped America in interaction with its liberal and democratic features, we can make more sense of a wide range of inegalitarian policies newly contrived after 1870 and perpetuated through much of the twentieth century. Those policies were dismantled only through great struggles, aided by international pressures during World War II and the Cold War; and it is not clear that these struggles have ended. The novelties in the policies and scientific doctrines of the Gilded Age and Progressive Era should alert us to the possibility that new intellectual systems and political forces defending racial and gender inequalities may yet gain increased power in our own time.

The civil rights reforms of the 1960s and 1970s are not as seriously threatened today as were the civil rights measures of the 1860s in the 1890s. Yet leading scholars like Richard Epstein now argue that the nation should eliminate all race-conscious laws, even the 1964 Civil Rights Act, in favor of programs of black self-help in the marketplace—precisely the position many nineteenth-century "liberals" used to justify abandoning Reconstruction (Epstein 1992). Also like these nineteenth-century predecessors, Epstein ultimately grounds his laissez-faire views not so much on a doctrine of human rights as on evolutionary biology, undaunted by how others then and now have used such views to explain racial as well as economic inequalities (Epstein 1985, 341, n. 19; Fairchild 1991). But though this blend of economics and sociobiology has disturbing precursors, some influential contemporary black leaders, such as Justice Clarence Thomas and economist Thomas Sowell, like Booker T. Washington before them, are aligned with such "self-help" views.

Racial, nativist, and religious tensions are also prominent in American life, as the Buchanan and Duke campaigns, the Christian Coalition, the Los Angeles riots, the English-Only agitation, the popularity of anti-Japanese novels, renewed patterns of residential segregation, and the upsurge of separatist ideologies among many younger minority scholars all indicate. The discourse about the "ethno-underclass" is particularly striking, for as Lawrence Fuchs notes, poor urban minorities are often portrayed as historically and socially conditioned to possess foreign moral values (1990, 487–89). The political message that these accounts convey often resembles, however unintentionally, that of Lodge's similar characterizations of undesirable "races": these groups appear so irreparably different and dangerous that they do not merit equal status in the political community.

It is too early to assess the significance of these aspects of current American life. The achievements of Americans in building a more inclusive democracy certainly provide reasons to believe that illiberal forces will not prevail. But just as we can better explain the nation's past by recognizing how and why liberal democratic principles have been contested with frequent success, we will better understand the present and future of American politics if we do not presume they are rooted in essentially liberal or democratic values and conditions. Instead, we must analyze America as the ongoing product of often conflicting multiple traditions.

Notes

1. The percentage varies according to whether one dates the United States from 1776, the Declaration of Independence, or 1789, the ratified Constitution. State policies prior to 1789 on the whole made nonwhites and women ineligible for full citizenship. Women could always formally be U.S. citizens, but they were almost universally denied the vote until 1920, making them clearly second-class citizens. Other overt legal discriminations on their political and economic rights continued through the 1960s. Naturalization was confined to whites from 1790 through 1868 and closed to most Asian nationals until 1952. By then, the national origins quota system of immigration restrictions, enacted in the 1920s, prevented most Asians and many southern Europeans from coming to the United States and becoming permanent residents or citizens, explicitly because of their original nationality or ethnicity. That system was not repealed until 1965. Despite formal constitutional guarantees enacted in the mid-1860s, blacks were also widely denied basic rights of citizenship until the 1964 Civil Rights Act and the 1965 Voting Rights Act (Higham 1975, 29–66; Kettner 1978, 287–322; Smith 1989). Thus, though the specifics changed, denials of access to full citizenship based explicitly on race, ethnicity, or gender always denied large majorities of the world's population any opportunity for U.S. citizenship up to 1965. That represents about 83% of the nation's history since the Constitution, 88% since the Declaration of Independence. If, controversially, one assumes that women became full citizens with the vote in 1920, then a majority of the domestic adult population became legally eligible for full citizenship then. This still means that a majority of domestic adults were ineligible for full citizenship on racial, ethnic, or gender grounds for about two-thirds of U.S. history (from either starting point).

2. Orren (1991), a major alternative critique of Tocquevillian accounts, shows ascriptive inegalitarian labor systems long prevailed even among white men.

3. From early on, many American intellectuals and politicians believed that "like the Chain of Being, the races of man consisted of an ordered hierarchy" (Haller 1971, 11; Russett 1989, 201–3). Some believed in a natural order of rank among the races, some that cultures fell into a higher and lower levels of civilization. Most thought race and culture linked. Scholars disagreed about the relative ranks of Asiatics, blacks, Native Americans, and other races and cultures, but these gradations mattered less than the supremacy of whites over nonwhites. Mulattoes, for example, were legally treated as an intermediate racial group in antebellum America, but by the 1850s whites began to reduce their status to that of "pure" blacks (Williamson 1980).

4. A tradition here is comprised by (1) a worldview or ideology that defines basic political and economic institutions, the persons eligible to participate in them, and the roles or rights to which they are entitled and (2) institutions and practices embodying and reproducing those precepts. Hence traditions are not *merely* sets of ideas. The liberal tradition involves limited government, the rule of law protecting individual rights, and a market economy, all officially open to all minimally rational adults. The republican tradition is grounded on popular sovereignty exercised via institutions of mass self-governance. It includes an ethos of civic virtue and economic regulation for the public good. Adherents of what I term ascriptive Americanist traditions believe true Americans are in some way "chosen" by God, history, or nature to possess superior moral and intellectual traits, often associated with race and gender. Hence many Americanists believe that nonwhites and women should be governed as subjects or second-class citizens, denied full market rights, and sometimes excluded from the nation altogether. My thesis—that an evolving mix of these traditions is visible in America's political culture, institutions, and the outlooks of Americans of *all* backgrounds—is indebted to Orren and Skowronek 1993.

5. Other major Tocquevillian works include Daniel

Boorstin's (1953) *Genius of American Politics* and S. M. Lipset's (1963) *First New Nation*.

6. As a full survey of pertinent works is impossible, I shall note some broad categories of scholarship in which most participants employ a misleading Tocquevillian framework, focusing on recent general discussions of American political culture and citizenship, where the limits of Tocquevillian premises are most damaging.

7. This Tocquevillian outlook has since been endorsed by writers as different as Samuel P. Huntington, who wrote in 1981 that the United States is virtually unique because the "political ideas of the American Creed"—not ancestry, ethnicity, religion or culture—have been the basis of American national identity, and Michael Walzer, who in 1990 endorsed Gleason's formulation and added that in America, with "severe but episodic exceptions," tolerance "has been the cultural norm." Walzer reached this conclusion while setting aside discussion of blacks and, implicitly, women's second-class citizenship as well (Huntington 1981, 23–25; Walzer 1990, 597–98, 610–11 [including n. 30]).

8. Several earlier works on citizenship employed frameworks closer to the one developed here. Hans Kohn's (1957) *American Nationalism* analyzed American nationality in terms of three similar foundations: the Enlightenment tradition of liberty, federal republicanism, and the interaction of the predominant Anglo–American cultural tradition with those of other national origins (pp. 9, 135, 165, 173, 252, n. 45). But writing before Bailyn and Pocock, Kohn treated federal republicanism as essentially a structural determinant of American nationality and saw its ideological content as derived chiefly from liberalism. And though Kohn recognized that the United States had often demanded assimilation into a "distinct national identity" built on English roots, his focus on the English commitment to liberty led him to downplay how illiberal–exclusivist this conception often was (pp. 13, 21, 28, 165–69). Mark Roelofs's (1957) *Tension of Citizenship* analyzed citizenship generally—and American citizenship incidentally—in terms of three patterns focusing on (1) "pride and participation" in the "communal life of the civic republic"; (2) "loyalty and service" to an "organic community"; and (3) individualistic and universalistic defiance to claims of particular communities and concern for protection of personal privacy. Though he labels these three "Greek," "Hebraic," and "Christian–Roman," they resemble the civic republican, ascriptive Americanist, and Enlightenment liberal elements I argue to be the chief original ideological traditions shaping American civic culture. Roelofs, however, conforms to orthodoxy in emphasizing the dominance of individualistic, rationalistic Enlightenment liberalism in America (pp. 31, 37, 76, 116–18, 125–31, 150–65). Yehoshua Arieli (1964) also chiefly identified American nationality with liberal republican political principles. But he discerned an "awareness of belonging to a national organic community whose values are to a certain degree not transferable." Thus he saw the "tension created by these two competing types of national consciousness" as perhaps the chief determinant of the "structure and course of American nationalism" (pp. 29–30). The present argument is partly an elaboration and justification of that insight. James Kettner (1978) published a seminal study of citizenship laws that attended to the status of women, blacks, and Native Americans. But like Myrdal, Kettner treated liberal and republican ideals as Americans' genuine beliefs and racially inegalitarian ones as simply "deep-seated prejudices," not explicit ideologies (pp. 3, 10, 349–51). More recent writers have combined the republican and religious revisions of Hartz by identifying three strands in American nationality, placing a "biblical" or "Christian" tradition alongside civic republican and individualistic liberal ones (Bellah et al., 1985; Kloppenberg 1987). But religious elements are better seen as bound up with all three of the traditions identified here. When they are taken as grounds for denying citizenship, as in Protestant nativism, they are closest to ascriptive Americanism; but religion is not the only source of Americanism, nor can Americanists claim to be the "authentic" voices of American religiosity. Sinopoli (1992) analyzes American citizenship only in terms of liberalism and republicanism, ignoring issues of racial, ethnic, and sexual civic statuses entirely.

9. I also agree that tensions between liberal and democratic ideas and institutions have been vital factors in American history, visible, for example, in the great struggles between the defenders of property rights and populist and labor movements. Those conflicts have, however, also always involved battles over the nation's racial, ethnic, and gender ordering.

10. From colonial times, American elites studied European naturalists' classifications of humanity into races, whose distinctive moral and intellectual characteristics created, at least implicitly, a rank order among them. Johann Blumenbach designated the "Caucasian" race as the original and "most beautiful" human race, of which others were progressive "degenerations" (Haller 1971, 5). In the antebellum period, Blumenbach's work was a reference for the "American School" of ethnology, led by physicians Samuel Morton, Josiah Nott, and John Van Evrie and supported by Harvard biologist Louis Aggasiz. Their apparent evidence of biological racial inequalities made racial exploitation of blacks and Native Americans seem reasonable to many (Haller 1971, 6–10; Horsman 1981, 125–35; Lofgren 1987, 99–101). By 1870, Americans had also long entertained doctrines, favored by northern European historians and philosophers, holding that the "Teutonic" and "Anglo–Saxon" peoples had been historically conditioned to be specially capable of self-governance and hence of paternal governance over lesser peoples. These views were echoed even by Whig politicians like Daniel Webster and romantic intellectuals like Ralph Waldo Emerson, who opposed slavery and extermination of the native tribes (Gossett 1963, 84–100; Horsman 1981, 9–42, 158–86; Howe 1979, 38–40; Jordan 1968, 339–41).

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DICTATORSHIP, DEMOCRACY, AND DEVELOPMENT

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Under anarchy, uncoordinated competitive theft by "roving bandits" destroys the incentive to invest and produce, leaving little for either the population or the bandits. Both can be better off if a bandit sets himself up as a dictator—a "stationary bandit" who monopolizes and rationalizes theft in the form of taxes. A secure autocrat has an encompassing interest in his domain that leads him to provide a peaceful order and other public goods that increase productivity. Whenever an autocrat expects a brief tenure, it pays him to confiscate those assets whose tax yield over his tenure is less than their total value. This incentive plus the inherent uncertainty of succession in dictatorships imply that autocracies will rarely have good economic performance for more than a generation. The conditions necessary for a lasting democracy are the same necessary for the security of property and contract rights that generates economic growth.

In my student days, in reading Edward Banfield's (1958) account of the beliefs of the people in a poor village in Southern Italy, I came upon a remarkable statement by a village monarchist. He said, "Monarchy is the best kind of government because the King is then owner of the country. Like the owner of a house, when the wiring is wrong, he fixes it" (p. 26). The villager's argument jarred against my democratic convictions. I could not deny that the owner of a country would have an incentive to make his property productive. Could the germ of truth in the monarchist's argument be reconciled with the case for democracy?

It is only in recent years that I have arrived at an answer to this question. It turns out that for a satisfactory answer one needs a new theory of dictatorship and democracy and of how each of these types of government affects economic development. Once this new theory is understood, one can begin to see how autocracies and democracies first emerge. I shall set out this conception in a brief and informal way and use it to explain some of the most conspicuous features of historical experience.

The starting point for the theory is that no society can work satisfactorily if it does not have a peaceful order and usually other public goods as well. Obviously, anarchic violence cannot be rational for a society: the victims of violence and theft lose not only what is taken from them but also the incentive to produce any goods that would be taken by others. There is accordingly little or no production in the absence of a peaceful order. Thus there are colossal gains from providing domestic tranquility and other basic public goods. These gains can be shared in ways that leave everyone in a society better off. Can we conclude that because everyone could gain from it, a peaceful order emerges by voluntary agreement?

From the logic of the matter, we should expect that in small groups a generally peaceful order will normally emerge by voluntary agreement but that in large populations it will not. The key to the matter is that each individual bears the full costs or risks of anything he or she does to help establish a peaceful order or to provide other public goods but receives only a share of the benefits. In a tiny group, such as a hunter–gatherer band, each person or family will obtain a significant share of the benefits of a peaceful order, and the net advantages of such an order are so great that even a single family's share of the gains can easily outweigh the sacrifices needed to obtain it. Moreover, when there are only a few, the welfare of each noticeably depends on whether each of the others acts in a group-oriented way. Thus each family, by making clear that cooperation by another will bring forth its cooperation but that noncooperation will not, can increase the likelihood that another will match its behavior, thereby increasing the incentive each has to act in the group interest. The theoretical prediction that sufficiently small groups can often organize for collective action is corroborated by countless observations (Olson 1965).

This prediction is also in accord with the anthropological observations of the most primitive societies. The simplest food-gathering and hunting societies are normally made up of bands that have, including the children, only about 50 or 100 people. In other words, such a band will normally contain only a few families that need to cooperate. Anthropologists find that primitive tribes normally maintain peace and order by voluntary agreement, and that is to some extent what Tacitus, Caesar, and other classical writers observed among the less advanced Germanic tribes. The most primitive tribes tend to make all important collective decisions by consensus, and many of them do not even have chiefs. When a band becomes too large or disagreement is intense, the band may split, but the new bands normally also make decisions by unanimous consent. If a tribe is in the hunting-and-gathering stage, there is also little or no incentive for anyone to subjugate another tribe or to keep slaves, since captives cannot generate enough surplus above subsistence to justify the costs of guarding them.[1] Thus within the most primitive tribes of preagricultural history, the logical presumption that the great gains from a peaceful order can be achieved by voluntary agreement appears to hold true.

Once peoples learned how to raise crops effectively, production increased, population grew, and large populations needed governments. When there is a large population, the same logic that shows why small groups can act consensually in their common interest, tells us that voluntary collective action *cannot* obtain the gains from a peaceful order or other public goods, even when the aggregate net gains from the provision of basic public goods are large.[2] The main reason is that the typical individual in a society with, say, a million people will get only about one-millionth of the gain from a collective good, but will bear the whole cost of whatever he or she does to help provide it, and therefore has little or no incentive to contribute to the provision of the collective good. There is by now a huge theoretical and empirical literature on this point, and the great preponderance of this literature agrees that, just as small groups can usually engage in spontaneous collective action, very large groups are not able to achieve collective goals through voluntary collective action.[3]

Thus we should not be surprised that while there have been lots of writings about the desirability of "social contracts" to obtain the benefits of law and order, no one has ever found a large society that obtained a peaceful order or other public goods through an agreement among the individuals in the society.

THE FIRST BLESSING OF THE INVISIBLE HAND

Why, then, have most populous societies throughout history normally avoided anarchy? An answer came to me by chance when reading about a Chinese warlord (see Sheridan 1966). In the 1920s, China was in large part under the control of various warlords. They were men who led some armed band with which they conquered some territory and who then appointed themselves lords of that territory. They taxed the population heavily and pocketed much of the proceeds. The warlord Feng Yu-hsiang was noted for the exceptional extent to which he used his army for suppressing bandits and for his defeat of the relatively substantial army of the *roving bandit*, White Wolf. Apparently most people in Feng's domain found him much preferable to the roving bandits.

At first, this seems puzzling: Why should warlords, who were *stationary bandits* continuously stealing from a given group of victims, be preferred, by those victims, to roving bandits who soon departed? The warlords had no claim to legitimacy and their thefts were distinguished from those of roving bandits only because they took the form of continuing taxation rather than occasional plunder.

In fact, if a roving bandit rationally settles down and takes his theft in the form of regular taxation and at the same time maintains a monopoly on theft in his domain, then those from whom he exacts taxes will have an incentive to produce. The rational stationary bandit will take only a *part* of income in taxes, because he will be able to exact a larger total amount of income from his subjects if he leaves them with an incentive to generate income that he can tax.

If the stationary bandit successfully monopolizes the theft in his domain, then his victims do not need to worry about theft by others. If he steals only through regular taxation, then his subjects know that they can keep whatever proportion of their output is left after they have paid their taxes. Since all of the settled bandit's victims are for him a source of tax payments, he also has an incentive to prohibit the murder or maiming of his subjects. With the rational monopolization of theft—in contrast to uncoordinated competitive theft—the victims of the theft can expect to retain whatever capital they accumulate out of after-tax income and therefore also have an incentive to save and to invest, thereby increasing future income and tax receipts. The monopolization of theft and the protection of the tax-generating subjects thereby eliminates anarchy. Since the warlord takes a part of total production in the form of tax theft, it will also pay him to provide other public goods whenever the provision of these goods increases taxable income sufficiently.

In a world of roving banditry there is little or no incentive for anyone to produce or accumulate anything that may be stolen and, thus, little for bandits to steal. Bandit rationality, accordingly, induces the bandit leader to seize a given domain, to make himself the ruler of that domain, and to provide a peaceful order and other public goods for its inhabitants, thereby obtaining more in tax theft than he could have obtained from migratory plunder. Thus we have "the first blessing of the invisible hand": the rational, self-interested leader of a band of roving bandits is led, as though by an invisible hand, to settle down, wear a crown, and replace anarchy with government. The gigantic increase in output that normally arises from the provision of a peaceful order and other public goods gives the stationary bandit a far larger take than he could obtain without providing government.

Thus government for groups larger than tribes normally arises, not because of social contracts or voluntary transactions of any kind, but rather because of rational self-interest among those who can organize the greatest capacity for violence. These violent entrepreneurs naturally do not call themselves bandits but, on the contrary, give themselves and their descendants exalted titles. They sometimes even claim to rule by divine right. Since history is written by the winners, the origins of ruling dynasties are, of course, conventionally explained in terms of lofty motives rather than by self-interest. Autocrats of all kinds usually claim that their subjects want them to rule and thereby nourish the unhistorical assumption that government arose out of some kind of voluntary choice. (These claims have an echo in some literature in the "transactions costs" tradition that attempts to explain the emergence of various kinds of governments partly or wholly through voluntary contracts and the costs of the transactions associated with them. See Barzel 1991; Kiser and Barzel 1991; North 1981; North and Thomas 1973.)[4]

Any individual who has autocratic control over a country will provide public goods to that country because he has an "encompassing interest" in it.[5] The extent of the encompassing interest of an officeholder, political party, interest group, monarch, or any other partial or total "owner" of a society varies with the size of the stake in the society. The larger or more encompassing the stake an organization or individual has in a society, the greater the incentive the organization or individual has to take action to provide public goods for the society. If an autocrat received one-third of any increase in the income of his domain in increased tax collections, he would then get one-third of the benefits of the public goods he provided. He would then have an incentive to provide public goods up to the point where the national income rose by the reciprocal of one-third, or three, from his last unit of public good expenditure. Though the society's income and welfare would obviously be greater from a larger expenditure on public goods, the gain to society from the public goods that a rational self-interested autocrat provides are nonetheless often colossal. Consider, for example, the gains from replacing a violent anarchy with a minimal degree of public order.

From history, we know that the encompassing interest of the tax-collecting autocrat permits a considerable development of civilization. From not long after the first development of settled agriculture until, say, about the time of the French Revolution, the overwhelming majority of mankind was subject to autocracy and tax theft. History until relatively recent times has been mostly a story of the gradual progress of civilization under stationary bandits interrupted by occasional episodes of roving banditry. From about the time that Sargon's conquests created the empire of Akkad until, say, the time of Louis XVI and Voltaire, there was an impressive development of civilization that occurred in large part under stationary banditry.[6]

THE GRASPING HAND

We can now begin to reconcile the village monarchist's insight and the foregoing argument with the case for democracy. Though the village monarchist was right in saying that the absolute ruler has as much incentive to fix what needs repair as the owner of a house, his analogy is nonetheless profoundly misleading. The autocrat is not in a position analogous to the owner of a single house or even to the owner of all housing, but rather to the owner of *all* wealth, both tangible and human, in a country. The autocrat does indeed have an incentive to maintain and increase the productivity of everything and everyone in his domain, and his subjects will gain from this. But he also has an incentive to charge a *monopoly* rent and to levy this monopoly charge on *everything*, including human labor.

In other words, the autocratic ruler has an incentive to extract the maximum possible surplus from the whole society and to use it for his own purposes. Exactly the *same* rational self-interest that makes a roving bandit settle down and provide government for his subjects also makes him extract the maximum possible amount from the society for himself. He will use his monopoly of coercive power to obtain the maximum take in taxes and other exactions.

The consumption of an autocratic ruler is, moreover, not limited by his personal capacities to use food, shelter, or clothing. Though the pyramids, the palace of Versailles, the Taj Mahal, and even Imelda Marcos's three thousand pairs of shoes were expensive, the social costs of autocratic leaders arise mostly out of their appetites for military power, international prestige, and larger domains. It took a large proportion of the total output of the Soviet Union, for example, to satisfy the preferences of its dictators.[7]

Some writers use the metaphor of the "predatory state" but this is misleading, even for autocracies. As we saw earlier, a stationary bandit has an encompassing interest in the territory he controls and accordingly provides domestic order and other public goods. Thus he is not like the wolf that preys on the elk, but more like the rancher who makes sure that his cattle are protected and given water. The metaphor of predation obscures the great superiority of stationary banditry over anarchy and the advances of civilization that have resulted from it. No metaphor or model of even the autocratic state can therefore be correct unless it simultaneously takes account of the stationary bandit's incentive to provide public goods at the same time that he extracts the largest possible net surplus for himself.

Although the forms that stationary banditry has taken over the course of history are diverse, the essence of the matter can be seen by assuming that the autocrat gets all of his receipts in the form of explicit taxation. The rational autocrat will devote some of the resources he obtains through taxation to public goods but will impose far higher tax rates than are needed to pay for the public goods since he also uses tax collections to maximize his net surplus. The higher the level of provision of public goods, given the tax rate, the higher the society's income and the yield from this tax rate. At the same time, the higher the tax rate, given the level of public-good provision, the lower the income of society, since taxes distort incentives.

So what tax rate and what level of public good provision will the rational self-interested autocrat choose? Assume for the moment that the autocrat's level of public-good expenditure is given. As Joseph Schumpeter (1991) lucidly pointed out, and Ibn Kalduhn (1967) sensed much earlier,[8] tax receipts will (if we start with low taxation) increase as tax rates increase, but after the revenue-maximizing rate is reached, higher tax rates distort incentives and reduce income so much that tax collections fall. The rational self-interested autocrat chooses the revenue-maximizing tax rate.

Though the amount collected at any tax rate will vary with the level of public-good provision, the revenue-maximizing tax *rate* for the autocrat should not. This optimal tax rate determines exactly how encompassing the interest of the autocrat in the society is; that is, it determines what share of any increase in the national income he receives. He will then spend money on public goods up to the point where his last dollar of expenditure on public goods generates a dollar's increase in his *share* of the national income. At this point, the gain to society will, as we know, be the reciprocal of his share.

Though the subjects of the autocrat are better off than they would be under anarchy, they must endure taxes or other impositions so high that, if they were increased further, income would fall by so much that even the autocrat, who absorbs only a portion of the fall in income in the form of lower tax collections, would be worse off.

There is no lack of historical examples in which autocrats for their own political and military purposes collected as much revenue as they possibly could. Consider the largest autocratic jurisdictions in Western history. The Bourbon kings of France were (especially on the eve of the French Revolution) collecting all they could in taxes. The Hapsburg kings of Spain did the same. The Roman Empire ultimately pushed its tax rates at least to the revenue-maximizing level.

THE REACH OF DICTATORSHIPS AND DEMOCRACIES COMPARED

How would government by a rational self-interested autocrat compare with a democracy? Democracies vary so much that no one conclusion can cover all cases. Nonetheless, many practical insights can be obtained by thinking first about one of the simplest democratic situations. This is a situation in which there are two candidates for a presidency or two well-disciplined parties seeking to form the government. This simplifying assumption will be favorable to democratic performance, for it gives the democracy an "encompassing" interest rather like the one that motivates the stationary bandit to provide some public goods. I shall make the opposite assumption later. But throughout, I shall avoid giving democracy an unfair advantage by assuming better motivation. I shall impartially assume that the democratic political leaders are just as self-interested as the stationary bandit and will use any expedient to obtain majority support.

Observation of two-party democracies tells us that incumbents like to run on a "you-never-had-it-so-good" record. An incumbent obviously would not leave himself with such a record if, like the self-interested autocrat, he took for himself the largest possible net surplus from the society. But we are too favorable to democracy if we assume that the incumbent party or president will maximize his chances of reelection simply by making the electorate as a whole as well-off as possible.

A candidate needs only a majority to win, and he might be able to "buy" a majority by transferring income from the population at large to a prospective majority. The taxes needed for this transfer would impair incentives and reduce society's output just as an autocrat's redistribution to himself does. Would this competition to buy votes generate as much distortion of incentives through taxation as a rational autocracy does? That is, would a vote-buying democratic leader, like the rational autocrat, have an incentive to push tax rates to the revenue-maximizing level?

No. Though both the majority and the autocrat have an encompassing interest in the society because they control tax collections, the majority in addition earns a significant share of the market income of the society, and this gives it a more encompassing interest in the productivity of the society. The majority's interest in its market earnings induces it to redistribute less to itself than an autocrat redistributes to himself. This is evident from considering an option that a democratic majority would have if it were at the revenue-maximizing tax rate. At the revenue-maximizing tax rate, a minuscule change in the tax rates will not alter tax collections. A minuscule *increase* in the tax rate will reduce the national income by enough so that even though a larger percentage of income is taken in taxes, the amount collected remains unchanged, and a tiny *reduction* in the tax rate will increase the national income so much that even though a smaller percentage is taken in taxes, receipts are unchanged. This is the optimal tax rate for the autocrat because changes in the national income affect his income only by changing tax collections.

But a majority at the revenue-maximizing tax rate is bound to increase its income from a *reduction* in tax rates: when the national income goes up, it not only, like the autocrat, collects taxes on a larger national income but also earns more income in the market. So the optimal tax rate for it is bound to be lower than the autocrat's. The easiest arithmetic example comes from supposing that the revenue-maximizing tax rate is one-third and that the majority earns one-third of the national income in the marketplace. The rational autocrat will then find that the last dollar in taxes that he collects reduces the national income by three dollars. One-third of this loss is his loss, so he just breaks even on this last dollar of tax collection and is at his revenue-maximizing rate. But if a majority mistakenly chose this same tax rate, it would be hurting itself, for it would lose two dollars (the same dollar lost by the autocrat plus one dollar of market income) from the last dollar it collected in taxes. Thus a majority would maximize its total income with a lower tax rate and a smaller redistribution to itself than would be chosen by an autocrat.[9]

More generally, it pays a ruling interest (whether an autocrat, a majority, or any other) to stop redistributing income to itself when the national income falls by the reciprocal of the share of the national income it receives. If the revenue-maximizing tax rate were one-half, an autocrat would stop increasing taxes when the national income fell by two dollars from his last dollar of tax collection. A majority that, say, earned three-fifths of the national income in the market and found it optimal to take one-fifth of the national income to transfer to itself would necessarily be reducing the national income by five-fourths, or $1.25, from the last dollar that it redistributed to itself. Thus the more encompassing an interest—the larger the share of the national income it receives taking all sources together—the less the social losses from its redistributions to itself. Conversely, the narrower the interest, the less it will take account of the social costs of redistributions to itself.

This last consideration makes it clear why the assumption that the democracy is governed by an encompassing interest can lead to much-too-optimistic predictions about many real-world democracies. The small parties that often emerge under proportional representation, for example, may encompass only a tiny percentage of a society and therefore may have little or no incentive to consider the social cost of the steps they take on behalf of their narrow constituencies. The special interest groups that are the main determinant of what government policies prevail in the particular areas of interest to those interest groups have almost no incentive to consider the social costs of the redistributions they obtain. A typical lobby in the United States, for example, represents less than 1% of the income-earning capacity of the country. It follows from the reciprocal rule that such a group has an incentive to stop arranging further redistributions to its clients only when the social costs of the redistribution become at least a hundred times as great as the amount they win in redistributional struggle (Olson 1982).

It would therefore be wrong to conclude that democracies will necessarily redistribute less than dictatorships. Their redistributions will, however, be shared, often quite unequally, by the citizenry. Democratic political competition, even when it works very badly, does not give the leader of the government the incentive that an autocrat has to extract the maximum attainable social surplus from the society to achieve his personal objectives.

LONG LIVE THE KING

We know that an economy will generate its maximum income only if there is a high rate of investment and that much of the return on long-term investments is received long after the investment is made. This means that an autocrat who is taking a long view will try to convince his subjects that their assets will be permanently protected not only from theft by others but also from expropriation by the autocrat himself. If his subjects fear expropriation, they will invest less, and in the long run his tax collections will be reduced. To reach the maximum income attainable at a given tax rate, a society will also need to enforce contracts, such as contracts for long-term loans, impartially; but the full gains are again reaped only in the long run. To obtain the full advantage from long-run contracts a country also needs a stable currency. A stationary bandit will therefore reap the maximum harvest in taxes—and his subjects will get the largest gain from his encompassing interest in the productivity of his domain—only if he is taking an indefinitely long view and only if his subjects have total confidence that their "rights" to private property and to impartial contract enforcement will be permanently respected and that the coin or currency will retain its full value.

Now suppose that an autocrat is only concerned about getting through the next year. He will then gain by expropriating any convenient capital asset whose *tax yield* over the year is less than its *total* value. He will also gain from forgetting about the enforcement of long-term contracts, from repudiating his debts, and from coining or printing new money that he can spend even though this ultimately brings inflation. At the limit, when an autocrat has no reason to consider the future output of the society at all, his incentives are those of a roving bandit and that is what he becomes.[10]

To be sure, the rational autocrat will have an incentive, because of his interest in increasing the investment and trade of his subjects, to promise that he will never confiscate wealth or repudiate assets. But the promise of an autocrat is not enforceable by an independent judiciary or any other independent source of power, because autocratic power by definition implies that there cannot be any judges or other sources of power in the society that the autocrat cannot overrule. Because of this and the obvious possibility that any dictator could, because of an insecure hold on power or the absence of an heir, take a short-term view, the promises of an autocrat are never completely credible. Thus the model of the rational self-interested autocrat I have offered is, in fact, somewhat too sanguine about economic performance under such autocrats because it implicitly assumed that they have (and that their subjects believe that they have) an indefinitely long planning horizon.

Many autocrats, at least at times, have had short time horizons: the examples of confiscations, repudiated loans, debased coinages, and inflated currencies perpetrated by monarchs and dictators over the course of history are almost beyond counting.

Perhaps the most interesting evidence about the importance of a monarch's time horizon comes from the historical concern about the longevity of monarchs and from the once-widespread belief in the social desirability of dynasties. There are many ways to wish a king well; but the king's subjects, as the foregoing argument shows, have more reason to be sincere when they say "long live the king." If the king anticipates and values dynastic succession, that further lengthens the planning horizon and is good for his subjects.

The historical prevalence of dynastic succession, in spite of the near-zero probability that the son of a king is the most talented person for the job, probably also owes something to another neglected feature of absolutisms. Any ruler with absolute power cannot, by definition, also have an independent source of power within the society that will select the next ruler and impose its choice upon the society. An independent capacity to install a new ruler would imply that this capacity can be used to remove or constrain the present autocrat. Thus, as is evident from modern dictatorships in Africa and Latin America, most dictatorships are by their nature especially susceptible to succession crises and uncertainty about the future. These uncertainties add to the problem of short time horizons that has just been described. In these circumstances, it may be advantageous to a society if a consensus emerges about who the next ruler will probably be, since this reduces the social losses arising from the absence in an autocracy of any independent power that could ensure a smooth succession. Given autocracy, then, dynastic succession can be socially desirable, both because it may reduce the likelihood of succession crises and because it may give monarchs more concern for the long run and the productivity of their societies.

DEMOCRACY, INDIVIDUAL RIGHTS, AND ECONOMIC DEVELOPMENT

We have seen that whenever a dictator has a sufficiently short time horizon, it is in his interest to confiscate the property of his subjects, to abrogate any contracts he has signed in borrowing money from them, and generally to ignore the long-run economic consequences of his choices. Even the ever-present possibility that an autocracy will come to be led by someone with a short time horizon always reduces confidence in investments and in the enforcement of long-run contracts. What do the individuals in an economy need if they are to have the maximum confidence that any property they accumulate will be respected and that any contracts they sign will be impartially enforced?

They need a secure government that respects individual rights. But individual rights are normally an artifact of a special set of governmental institutions. There is no private property without government! In a world of roving bandits some individuals may have possessions, but no one has a claim to private property that is enforced by the society. There is typically no reliable contract enforcement unless there is an impartial court system that can call upon the coercive power of the state to require individuals to honor the contracts they have made.

But individuals need their property and their contract rights protected from violation not only by other individuals in the private sector but also by the entity that has the greatest power in the society, namely, the government itself. An economy will be able to reap all potential gains from investment and from long-term transactions only if it has a government that is believed to be both strong enough to last and inhibited from violating individual rights to property and rights to contract enforcement. What does a society need in order to have a government that satisfies both of these conditions?

Interestingly, the conditions that are needed to have the individual rights needed for maximum economic development are exactly the same conditions that are needed to have a *lasting* democracy. Obviously, a democracy is not viable if individuals, including the leading rivals of the administration in power, lack the rights to free speech and to security for their property and contracts or if the rule of law is not followed even when it calls for the current administration to leave office. Thus the *same* court system, independent judiciary, and respect for law and individual rights that are needed for a lasting democracy are also required for security of property and contract rights.

As the foregoing reasoning suggests, the only societies where individual rights to property and contract are confidently expected to last across generations are the securely democratic societies. In an autocracy, the autocrat will often have a short time horizon, and the absence of any independent power to assure an orderly legal succession means that there is always substantial uncertainty about what will happen when the current autocrat is gone. History provides not even a single example of a long and uninterrupted sequence of absolute rulers who continuously respected the property and contract-enforcement rights of their subjects. Admittedly, the terms, tenures, and time horizons of democratic political leaders are perhaps even shorter than those of the typical autocrat, and democracies lose a good deal of efficiency because of this. But in the secure democracy with predictable succession of power under the rule of law, the adjudication and enforcement of individual rights is not similarly short-sighted. Many individuals in the secure democracies confidently make even very-long-term contracts, establish trusts for great-grandchildren, and create foundations that they expect will last indefinitely and thereby reveal that they expect their legal rights to be secure for the indefinite future.

Not surprisingly, then, capital often flees from countries with continuing or episodic dictatorships (even when these countries have relatively little capital) to the stable democracies, even though the latter are already relatively well supplied with capital and thus offer only modest rates of return. Similarly, the gains from contract-intensive activities such as banking, insurance, and capital markets are also mainly reaped by stable democracies like the United States, the United Kingdom, and Switzerland. Though experience shows that relatively poor countries can grow extraordinarily rapidly when they have a strong dictator who happens to have unusually good economic policies, such growth lasts only for the ruling span of one or two dictators. It is no accident that the countries that have reached the highest level of economic development and have enjoyed good economic performance across generations are all stable democracies. Democracies have also been about twice as likely to win wars as have dictatorships (Lake 1992).

THE IMPROBABLE TRANSITION

How do democracies emerge out of autocracies? It is relatively easy to see how autocratic government emerges and why it has been the predominant form of government since the development of settled agriculture: there is never a shortage of strong men who enjoy getting a fortune from tax receipts. It is much harder to see how democratic government can emerge out of autocracy.

It is a logical mistake to suppose that because the subjects of an autocrat suffer from his exactions, they will overthrow him. The same logic of collective action that ensures the absence of social contracts in the historical record whereby large groups agreed to obtain the advantages of government also implies that the masses will not overthrow an autocrat simply because they would be better off if they did so. Historical evidence from at least the first pharaohs through Saddam Hussein indicates that resolute autocrats can survive even when they impose heinous amounts of suffering upon their peoples. When they are replaced, it is for other reasons (e.g. succession crises) and often by another stationary bandit.[11] What special circumstances explain the cases where a more or less democratic[12] or at least pluralistic government emerges out of an autocracy?

One obvious special circumstance is that, partly for the reasons just set out, the richest countries are democracies, and democracies have usually prevailed in the competitions with their major autocratic competitors, whether fascist or communist. The triumphant democracies have sometimes encouraged or subsidized transitions to democracy in other countries. In some cases, such as Germany, Japan, and Italy after World War II, the victorious democracies more or less demanded democratic institutions as a price for giving independence to the vanquished nations. The theoretical challenge is to explain not these transitions but rather those that are entirely internal and spontaneous.

Easy as it would be to argue that the initially or spontaneously democratic countries were blessed with democratic cultures or selfless leaders, this would be an ad hoc evasion. The obligation here is to explain the spontaneous transitions to democracy from the same parsimonious theory that has been used in the rest of this essay.

The theory suggests that the key to an explanation of the spontaneous emergence of democracy is the *absence* of the commonplace conditions that generate autocracy. The task is to explain why a leader who organized the overthrow of an autocrat would not make himself the next dictator or why any group of conspirators who overthrew an autocrat would not form a governing junta. We have seen that autocracy is a most profitable occupation and that the authors of most coups and upheavals have appointed themselves dictators. So the theory here predicts that democracy would be most likely to emerge spontaneously when the individual or individuals or group leaders who orchestrated the overthrow of an autocracy could not establish another autocracy, much as they would gain from doing so. We can deduce from the theory offered here that autocracy is prevented and democracy permitted by the accidents of history that leave a balance of power or stalemate—a dispersion of force and resources that makes it impossible for any one leader or group to overpower all of the others.

But this deduction does *not* give us any *original* conclusion: rather, it points directly toward one of the major inductive findings in some of the literature in history and in political science on the emergence of democracy. If the theory here is right, there must be a considerable element of truth in the famous "Whig interpretation" of British history and in the explanations of democracy offered by political scientists such as Robert Dahl (1971) and, especially, Tatu Vanhanen (1989). If the theory offered here is right, the literature that argues that the emergence of democracy is due to historical conditions and dispersions of resources that make it impossible for any one leader or group to assume all power is also right.

Yet it is also necessary to go back again to the theory for a crucial detail. Even when there is a balance of power that keeps any one leader or group from assuming total control of a large area or jurisdiction, the leader of each group may be able to establish himself as an autocrat of a small domain. A dispersion of power and resources over a large area can result in a set of small-scale autocracies but no democracy. If, however, the different contending groups are scrambled together over a wide and well-delineated domain, then small autocracies are not feasible. They may not be feasible also if each of the leaders capable of forming a small-scale autocracy believes that a domain of that small scale would not be viable, whether because of aggression by other autocrats or for any reason.

If scrambled constituencies or any other reason rules out division of a domain into miniautocracies, then the best attainable option for the leader of each group when there is a balance of power is power sharing. If no one leader can subdue the others or segregate his followers into a separate domain, then the alternative is either to engage in fruitless fighting or to work out a truce with mutual toleration. The provision of a peaceful order and other public goods will, in these circumstances, be advantageous for all of the groups; thus, the leaders of the different groups have an incentive to work out mutually satisfactory arrangements for the provision of such goods. Given peaceful conditions, there are great gains to leaders and other individuals in each group from being able to make mutually advantageous contracts with others and thereby a common interest in establishing a disinterested and independent judiciary. With several groups, it is not certain in advance how elections will turn out, yet each group can, by allying with other groups, ensure that no one other group will continually dominate elections. Thus elections as well as consensual agreements among the leaders of the different groups can be consistent with the interest of the leaders and members of each group.

Though there are a fair number of democracies, there have not been many spontaneous and entirely autonomous transitions from autocracy to democracy. Most of the democracies in the English-speaking world owed a good deal to the pluralism and democracy that emerged in late seventeenth-century Britain and thus they usually do not offer a completely independent test of the argument about the transition to democracy offered here.

Happily, the initial emergence of democracy with the Glorious Revolution of 1689 in England (and its very gradual transition from a democracy with a highly restricted franchise to universal suffrage) nicely fits the logic of the democratic transition predicted by the present theory. There were no lasting winners in the English civil wars. The different tendencies in British Protestantism and the economic and social forces with which they were linked were more or less evenly matched. There had been a lot of costly fighting and, certainly after Cromwell, no one had the power to defeat all of the others. The restored Stuart kings might have been able to do this, but their many mistakes and the choices that ultimately united almost all of the normally conflicting Protestant and other political tendencies against them finally led to their total defeat.

None of the victorious leaders, groups, or tendencies was then strong enough to impose its will upon all of the others or to create a new autocracy. None had any incentive to give William and Mary the power to establish one either. The best option available to each of the leaders and groups with power was to agree upon the ascendancy of a Parliament that included them all and to take out some insurance against the power of the others through an independent judiciary and a Bill of Rights. (The spread of the franchise is too long a story to tell here. But it is not difficult to see how, once the society was definitely nonautocratic and safely pluralist, additional groups could parlay the profitable interactions that particular enfranchised interests had with them—and the costs of suppression that they could force the enfranchised to bear—into a wider suffrage.)

With a carefully constrained monarchy, an independent judiciary, and a Bill of Rights, people in England in due course came to have a relatively high degree of confidence that any contracts they entered into would be impartially enforced and that private property rights, even for critics of the government, were relatively secure. Individual rights to property and contract enforcement were probably more secure in Britain after 1689 than anywhere else, and it was in Britain, not very long after the Glorious Revolution, that the Industrial Revolution began.[13]

Though the emergence of a democratic national government in the United States (and in some other areas of British settlement, such as Australia and Canada) was partly due to the example or influence of Great Britain, it also was due in part to the absence of any one group or colonial government that was capable of suppressing the others. The 13 colonies were different from one another even on such important matters as slavery and religion, and none of them had the power to control the others. The separate colonies had, in general, experienced a considerable degree of internal democracy under British rule, and many of the colonies were, because of the different religious and economic groups they contained, also internally diverse. Many of the authors of the U.S. Constitution were, of course, also profoundly aware of the importance of retaining a dispersion of power (checks and balances) that would prevent autocracy.

THE DIFFERENT SOURCES OF PROGRESS IN AUTOCRACIES AND DEMOCRACIES

Since human nature is profoundly complex and individuals rarely act out of unmixed motives, the assumption of rational self-interest that I have been using to develop this theory is obviously much too simple to do justice to reality. But the caricature assumption that I have been using has not only simplified a forbiddingly complex reality but also introduced an element of impartiality: the same motivation was assumed in all regimes. The results are probably also robust enough to hold under richer and more realistic behavioral assumptions.

The use of the same motivational assumption and the same theory to treat both autocracy and democracy also illuminates the main difference in the sources of economic growth and the obstacles to progress under autocracy and under democracy. In an autocracy, the source of order and other public goods and likewise the source of the social progress that these public goods make possible is the encompassing interest of the autocrat. The main obstacle to long-run progress in autocracies is that individual rights even to such relatively unpolitical or economic matters as property and contracts can never be secure, at least over the long run.

Although democracies can also obtain great advantages from encompassing offices and political parties, this is by no means always understood (Olson 1982, 1986); nor are the awesome difficulties in keeping narrow special interests from dominating economic policymaking in the long-stable democracy. On the other hand, democracies have the great advantage of preventing significant extraction of social surplus by their leaders. They also have the extraordinary virtue that the same emphasis on individual rights that is necessary to lasting democracy is also necessary for secure rights to both property and the enforcement of contracts. The moral appeal of democracy is now almost universally appreciated, but its economic advantages are scarcely understood.

Notes

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1. There is quantitative evidence from an exhaustive survey of ethnographic accounts showing that references to slaves are virtually absent in the accounts of the very most primitive peoples but rather common in more advanced agricultural societies (Hobhouse, Wheeler, and Ginsberg 1930). Slavery is unprofitable in hunting-gathering societies (Olson 1967).

2. Small tribes can sometimes form federations and thereby increase the number who can obtain collective goods through voluntary action (Olson 1965, 62–63). Some of the very earliest agricultural societies may have been of this character. But when the number of small groups itself becomes very large, the large-number problem is evident again and voluntary collective action is infeasible.

3. For citations to much of the best literature extending and testing the argument in *The Logic of Collective Action*, as well as for valuable new analyses, see Hardin 1982 and Sandler 1992.

4. This literature is most constructive and interesting, but to the extent to which it tries to explain government in terms of voluntary transactions, it is not convincing. North, while emphasizing transactions costs and contracts, also uses the notion of the "predatory state" and the logic of collective action in his account of the state, so his approach must be distinguished from Barzel's.

5. For the definition of an encompassing interest and evidence of its importance, see Olson 1982. The logical structure of the theory that encompassing interests will be concerned with the outcome for society whereas narrow groups will not is identical with the logic that shows that small groups can engage in voluntary collective action when large groups cannot.

6. Many of the more remarkable advances in civilization even in historic times took place in somewhat democratic or nondictatorial societies such as ancient Athens, the Roman Republic, the North Italian city-states, the Netherlands in the seventeenth century, and (at least after 1689) Great Britain. The explanation for the disproportionate representation of nonautocratic jurisdictions in human progress is presented later in the article.

7. The theory offered here applies to communist autocracies as much as to other types, though the theory needs to be elaborated to take account of the "implicit tax-price discrimination" pioneered by Joseph Stalin. This innovation enabled Stalinist regimes to obtain a larger proportion of social output for their own purposes than any other regimes had been able to do. This explained Stalin's success in making the Soviet Union a superpower and the great military capacity of many communist regimes. It also generated a unique dependence of the system on its management cadre, which ultimately proved fatal. For how the offered theory applies to communist autocracies and the societies in transition, see Clague and Rausser 1992, pref., chap. 4; Murrell and Olson 1991; Olson 1993.

8. Schumpeter's analysis is in his "Crisis of the Tax State," written in the highly taxed Austria-Hungarian Empire late in World War I; Ibn Kalduhn's is in his classic, *The Mugaddimah*.

9. A mathematical and a geometrical proof of this conclusion and an analysis of many other technical questions raised by the present theory is available on request.

10. When war erodes confidence about what the boundaries of an autocrat's domain will be, an autocrat's time horizon with respect to his possession of any given territory shortens—even if he believes that he will remain in control of some territory somewhere. In the limit, complete uncertainty about what territory an autocrat will control implies roving banditry. The advantages of stationary banditry over roving banditry are obviously greatest when there are natural and militarily defensible frontiers. Interestingly, the earliest states in history emerged mainly in what one anthropologist calls "environmentally circumscribed" areas, that is, areas of arable land surrounded by deserts, mountains, or coasts (see Carneiro 1970). The environmental circumscription not only provides militarily viable frontiers but also limits the opportunity for defeated tribes to flee to other areas in which they could support themselves (as Carneiro points out). This in turn means that the consensual democracy characteristic of the earliest stages of social evolution is, in these geographical conditions, replaced by autocratic states earlier than in other conditions.

11. For more examples of other types of reason, see Olson 1990.

12. In the interest of brevity, democracy is here defined as competitive elections, social pluralism, and the absence of autocracy, rather than in terms of universal suffrage. Although how a narrower suffrage turns into a wider suffrage can be explained by straightforward extensions of the logic of the theory offered here, developing these extensions and testing them against the historical evidence would not be a small undertaking.

13. For striking evidence on how the growth of cities was much greater in medieval and early modern Europe in democratic or less autocratic regimes, see DeLong and Schleifer 1992. In effect, the DeLong and Schleifer paper is a test of the advantages of democracy that I put forward.

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THE ELECTORAL FORTUNES OF LEGISLATIVE FACTIONS IN JAPAN

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The legislative factions of the Liberal Democratic Party of Japan are so autonomous that the LDP is typically viewed as a coalition of factions, rather than a unitary party. We focus on the electoral role of these factions, finding that the five main factions differed substantially in electoral success in the 1960–79 period, but have been so closely tied together in the 1980s that differences in their electoral fates are statistically indiscernible. In particular, we find that the so-called mainstream factions did consistently better than their nonmainstream rivals before 1980 but not after. We explain the lessening of interfactional differences in terms of a decentralization of fund-raising within factions, which tended to equalize factional war chests (on a per capita basis).

The legislative factions of the Liberal Democratic Party (LDP) of Japan are among the longest-lived and most institutionalized in the world. The factions predate the formation of the LDP in 1955 and are often taken as the "primary units" from which the LDP is constituted (Scalapino and Masumi 1962, 18–19, 54). Ward, for example, views the LDP as "a loose coalition of factions," while McNelly prefers an "amalgam of factions," and Ike, a "federation of factions" (Ike 1972, 81–83; McNelly 1984, 97; Ward 1968, 65, 68–69).

The autonomy of the LDP factions is nowhere more evident than in the conduct of elections, where competition among LDP candidates from different factions is often portrayed as fiercer and more consequential than interparty competition between the LDP and its various opponents. Nonetheless, the factions' electoral role is a bit shadowy: they do not put their names on the ballot, campaign as corporate entities, or take clear and public stands on the issues of the day. Chiefly, they act behind the scenes, helping their members secure governmental and party posts, the official LDP nomination, and money. The electoral importance of these factional activities is universally asserted but is hard to measure in any systematic way. The difficulty is greatest when it comes to money, because Japanese reporting requirements are so lax that no one takes official figures seriously.

We shall offer an indirect method of assessing the electoral impact of the LDP factions in the period 1960–90. Although one can say little about how much money a particular candidate got from his faction, one can identify both factional affiliations and vote totals for most LDP candidates. This is enough information to address an issue that, oddly enough given the centrality of factions in the literature on Japanese elections, no one has bothered to investigate systematically—the extent to which their fates are electorally distinct.

It is widely recognized that the LDP as a whole has good years and bad; partisan electoral tides do wash through the Japanese electorate (Kawato 1991). But are there factional eddies within the partisan tides? The question has not been asked. It is important not just on its own merits but also because it bears directly on how one views LDP factionalism—or so we shall argue.

We begin by reviewing the three most prominent explanations for the existence of factionalism in the LDP, offering our own view too. We then ask the main question of the paper—how far the electoral fates of the various LDP factions have been tied together. After discussing what one ought to expect, we investigate over-time patterns both in vote percentages and vote swings, explicitly controlling for the factional membership of the candidates. What we find, in brief, is that the five main LDP factions were clearly distinguishable in the electoral returns in the 1960–79 period but have been so closely tied together in the 1980s that differences in their electoral fates are statistically indiscernible. In particular, we find that the so-called mainstream factions did consistently better than their nonmainstream rivals before 1980 but not after.

We explain this pattern of evidence as follows. In the 1960s and early 1970s, a substantial proportion of campaign finance was raised by factional bosses. Each faction's electoral success was thus highly sensitive to the factional boss's success in fund-raising, something that was highly variable from year to year. In the late 1970s and early 1980s, the proportion of campaign funds raised by the factional boss declined markedly, with the individual member picking up the slack. This decentralization of fund-raising produced less variation among the factions' war chests (on a per capita basis) because how much each faction raised now depended on the average effort of many members rather than the single effort of the boss. Less variation in fund-raising in turn produced less variation in electoral performances.

Finally, we discuss some alternative interpretations of the data, noting in particular that any process that equalized factional resources might have equalized their electoral success. We look at two other electorally valuable resources (cabinet posts and endorsements) and show that neither equalized anywhere near the point at which electoral differences between the factions lessened.

WHY FACTIONS EXIST: SOME CURRENT VIEWS

The literature on Japanese politics offers three main views on the causes of factionalism in the LDP: one emphasizes Japanese cultural predispositions; one, competition for the LDP presidency; and one, adaptation to the Japanese electoral system of "middle-sized districts." We shall briefly review each of these positions.

The cultural explanation for LDP factionalism is extremely widespread. It consists, typically, of a recitation of the long history of endemic factionalism in Japanese society. Johnson for example, notes that the "most basic" cause of the LDP's factionalism "is the continuing ethos of patrimonial relations from Japan's feudal past and the tendency of all large Japanese organizations to structure themselves internally not into horizontal classes of members but into vertically divided competitive groups" (1990, 78). Ishida and Baerwald offer a similar view, alluding to the existence of factions among government officials from particular provinces, among army and navy officers, Imperial University of Tokyo alumni, and so on; Baerwald concludes with the comment that "to anticipate or to wish that factionalism could . . . be eliminated from the LDP . . . is to expect this political party to become something other than a Japanese organization" (Baerwald 1986, 17; Ishida 1971, 64–67).[1]

The second frequently encountered explanation for LDP factionalism focuses on competition for the LDP presidency, a post whose occupant becomes prime minister of Japan when the party has a majority, which it has had continuously since its inception. Most Japanese scholars mention competition for the LDP presidency as contributing to the maintenance of factions (see, e.g., Kitaoka 1985; Sato and Matsuzaki 1986; Tanaka 1986). Leiserson sees the presidential race as explaining why LDP factions are "institutionalized" rather than merely personal followings:

> The year after its formation, the LDP decided that its president would be chosen by vote, with LDP Diet members holding practically all the votes. Up until this radical innovation, and for a little while after it, factions in the LDP were more or less what factions in Japan—or anywhere—had always been: a nucleus of a few key lieutenants around a leader, with a rather unreliable following. But after this innovation was put into operation, . . . the factions became like army "divisions," headed by a "general" who was advised by his "General Staff." There were "line officers," fixed and known memberships, offices, publications, regular sources of funds, and so forth. In a word, the factions became institutionalized. (1968, 770–71).

Thayer offers a more extended version of the same view, concluding that "the greatest stimulus to the growth of factions in the present conservative party has been the party presidential elections" (1969, 20–26, quote from p. 21).

Many who emphasize the importance of the presidential race also have in mind the vast array of other party and governmental posts beyond the LDP presidency and Japanese premiership (e.g., Fukui 1978, 50; Leiserson 1968; Thayer 1969, 31–5). With many prizes to be handed out, the structure of the situation is more conducive to multifactionalism than it might have been were there only a single prize, the presidency, to be attained. Nonetheless, neither the argument focusing on pursuit of the presidency in particular nor the argument focusing on pursuit of "prestige" posts in general is complete without further information about the Japanese system. After all, the British Conservatives have had a comparable amount of posts to hand out over the decade of the 1980s (including the party leadership/premiership), yet they are not factionalized in anything like the same way.

A third explanation for LDP factionalism focuses on the Japanese electoral system. Okimoto, for example, argues as follows:

> Factions arise out of a combination of conditions, perhaps the most important being Japan's unusual system of multicandidacy election districts. In each district represented in the Lower House, anywhere from three to five seats are open, with each registered voter holding the right to cast only a single vote. . . . [This] gives rise to a situation in which LDP candidates wind up running as aggresively against each other as against candidates from other parties. It cannot help but foster fierce intraparty competition. Out of the crucible of this intense struggle for power emerges the phenomenon of LDP factions. (1988, 207)

Fukui offers a similar view, noting that the electoral system "contributes prominently to the growth of factionalism in the [LDP and Japan Socialist party]. An active member of the LDP's 1972 'mainstream' faction once declared that so long as he [was] forced to compete not only with opposition candidates but also with senior men from his own party, he need[ed] the support of his faction simply to survive" (1978, 50). The *Asahi Shimbun* expressed an even stronger view of the importance of the electoral system at one point in the 1960s, asserting that "if a single-member constituency [system] is created, . . . the factions will naturally disappear" (quoted in Thayer 1969, 141).[2]

We shall henceforth focus on the specifically political explanations of factionalism—those hingeing on intraparty competition for the LDP presidency and for seats in the Diet. We now offer our own synthesis of these (largely complementary) views.

FACTIONALISM AS AN ADAPTATION TO ELECTORAL STRUCTURE

Every party in the world has leaders and would-be leaders. Yet in very few does competition for the leadership lead to the formation of institutionalized factions lending substantial assistance to their members in the electoral arena. In the Anglo-American democracies, for example, competitors for leadership posts typically cobble their support coalitions together at the last moment, largely with promises of office; electoral help (in the form of an appearance in the constituency or an occasional contribution) is given but falls far short of that given by seekers after the LDP presidency. What makes the pursuit of leadership in the LDP so much different from its counterpart elsewhere? In our view, the key considerations in answering this question are (1) the rules under which the LDP president is chosen and (2) the rules under which Japanese Diet members are elected.

From 1956 to 1978, the LDP president was formally elected at the party's national convention by an electorate consisting mostly of LDP Diet members. Presidential aspirants thus had good reason to seek followers in the Diet. However, political parties in many other countries also confine the franchise in their leadership contests to parliamentarians yet have no factions—so this feature alone cannot explain LDP factionalism.

Few scholars mention the precise method of election used at the national convention. Possibly, this is because the LDP presidency has in practice often been decided by backroom negotiating, with the convention vote a mere formality. But this is no reason to ignore the formal electoral mechanism, since the backroom negotiations are undertaken in light of the process that will ensue should they break down.

From 1956 to 1978, the electoral process entailed a first ballot, on which a majority of votes was required to secure election, followed if necessary by a runoff between the top two vote-getters.[3] Duverger (1954) has provided substantial evidence that such electoral rules, when used in general elections, promote multipartism. Key's (1949) work on Southern politics has similarly shown that such rules, when used in the primary elections of a dominant party, produce multifactionalism. A fairly large body of empirical evidence thus suggests a correlation between runoffs and the fractionalization of electoral competition.

This correlation, however, is not a promising basis on which to explain LDP factionalism. Many parties use multiballot elections (with eventual requirements for majority support) to elect their leaders yet have little if any institutionalized factionalism. The use of runoffs cannot by itself explain the highly institutionalized form of Japanese factionalism.

Turning now to the rules governing election of Diet members, we focus on the exigencies of competition under Single Non-transferable Vote (SNTV). Because each voter has only one (nontransferable) vote to cast and members from the same party are ideologically more similar than are members from opposing parties, even the simplest spatial model suggests that intraparty competition for votes will be as important as—and possibly more important than—interparty competition. This accords with conventional wisdom in Japan: "Both the candidates and their campaign workers keep their hands off the domains of the opposition parties, where it is difficult to cut into the vote, but aim at the domains of the candidates of the same party. These are the so-called election blood feuds and they can be observed in each district throughout the nation" (Tsuchiya Shozo, quoted in Thayer 1969, 119). Moreover, quantitative evidence to back up spatial suspicions and conventional wisdom is not lacking. For example, of the 399 LDP incumbents defeated in the elections from 1960 to 1990, 48% were replaced by another LDP candidate.[4]

Because intraparty competition for votes is continual, LDP members constantly seek an edge over their party brethren. It is understandable then that competition for the LDP presidency should have taken the distinctly electoral form that it did: rank-and-file members need electoral assistance; and senior members with good connections can provide it, largely in the form of campaign finance and help in securing the LDP imprimatur.

Certainly a number of other features of LDP factionalism would merit notice in a more comprehensive anatomy, but we think the bare bones exposed here are the essential ones: competition for the LDP presidency leads, *given the rules of election both for that office* (the electorate consists mostly of Diet members who cast votes in a majority runoff election) *and for seats in the Diet* (SNTV in middle-sized districts), to the formation of institutionalized factions.

One direction in which research might head from the preliminary conclusion just reached is toward extensive comparison: a systematic reconnoitoring of the rules used to elect party leaders and their followers in various polities, together with a description of the degree and nature of party factionalism. But we shall head in a different, more intensive, direction. If LDP factions exist as such separate and autonomous actors and if such an important part of their existence centers on getting their members elected, what exactly do they do and to what effect?

EXPECTATIONS

We have already noted that factions can help their members in the electoral arena by competing for party and governmental posts, for the official LDP endorsement, and for money. Less importantly, factional leaders also give advice and make campaign appearances on behalf of their followers.

If factions influence elections largely through placing their members in desirable posts, securing the LDP endorsement, and providing money, then it is natural to look to these factors to explain any divergence in their electoral success. We shall first focus on money, arguably the most important of these three resources.

It is a shibboleth accepted by all observers and articulated by no less an authority than Tanaka Kakuei (the former prime minister convicted of accepting bribes from the Lockheed Corporation in the 1970s) that "politics requires power; power requires numbers; and numbers require money" (quoted in

Honzawa 1990, 195). How much LDP candidates typically spend is unknown, because the government imposes notoriously weak reporting requirements. Estimates range from 200 million to 1 billion yen per candidate, or about \$1.5–\$7.5 million at 135 yen to the dollar (Honzawa 1990; Iwai 1990; Sasago, Abe, and Muraoka 1990). By way of comparison, viable candidates for the U.S. House of Representatives are typically thought to have needed about \$300,000 in the mid-1980s in order to woo a constituency more populous than their Japanese counterpart's.

If money is so important and factional penetration of elections is primarily in terms of campaign contributions to their members, then presumably factions that had good fund-raising years will do well, while those that did not will do poorly. It is notoriously difficult to ascertain how much money each faction collects;[5] but if one cannot see any corporate identity to the factions in the electoral returns, then one is left with two hypotheses: either the factions are all equally good at collecting money (relative to the number of mouths they have to feed) or the importance of money is overrated.

The latter hypothesis seems unlikely, so consider the conditions under which the factions would be distinct or indistinct in terms of their ability to raise money in proportion to their numbers. Here we shall focus on a financial condition, namely, the proportions of campaign funds raised by the party, the factional boss, and the individual member. We shall argue that the lower the proportion of funds raised by the factional boss, the less distinct the factions will be in their per capita financial strength.

If most of the money in each faction member's war chest comes from the factional boss, then one would expect factions to be distinct in their ability to raise money. This conclusion does not require an assumption that different factional bosses are endowed with different innate fund-raising talents. No doubt they are; but over the long run, this probably just means that some factions will be larger than others. One expects, in other words, that the amount of money *per member* will be about the same across factions in equilibrium. If this expectation is inaccurate, then the conclusion that centralized factions will have different financial strengths follows directly. Even if the expectation is accurate, however, and factions *do* gather about the same amount of money per member over the long run, there is still room for year-to-year variation around this long-run average: the factional boss can have a bad fund-raising year, miscalculate the number of new candidates he can support, give too little to the needy and too much to the already-rich, and so forth. If campaign finance is highly centralized, these fluctuations around the average may, in any given year, be large enough to produce a noticeable divergence of fortunes between the factions.

Things are different if campaign finance is either dominated by the party or highly decentralized. In the first case, per capita war chests are equalized among the factions simply on the assumption that party funds are distributed more or less equally to each member. In the second case, per capita war chests tend to equalize because of the statistical "law of averages." Each faction's overall success in fund-raising is no longer determined by the efforts of one boss but by the average efforts of many members. Because the bad years of one member are balanced by the good years of another, differences between the per capita expenditure of the factions arise only if one or another faction systematically attracts better fund-raisers, a proposition for which there is no evidence in the literature.[6]

Turning to the literature, we find that one of the most important and visible recent claims about Japanese electoral politics is that fund-raising has become significantly decentralized in the LDP, starting in the mid-1970s (see Curtis 1988; Iwai 1990). This claim, which we shall call the *decentralization hypothesis*, has a clear implication in light of the foregoing discussion: the LDP factions should become less distinct electorally, starting at roughly the same time.[7] Iwai (1990) also suggests that prior to the 1990 election, fund-raising became more of a party and less of a factional concern; had such centralization occurred a decade earlier (something that no one currently claims), it would also have led to electorally less distinct factions.

DATA

Have the LDP factions differed in their electoral success? In order to get at this question, we analyze vote swings to individual LDP candidates in elections from 1960 to 1990. We restrict the cases included in the analysis in two primary ways. First, only *continuing* candidates (i.e., those who competed in the immediately preceding election) are included in any given year for the obvious reason that "swings" cannot be defined for first-time candidates.[8] Second, only members of the five big factions (those initiated by Sato, Kishi, Ikeda, Kono, and Miki) are included.[9] As these factions were presumably the most successful electorally, this restriction biases the analysis against finding any interfactional differences. Even with these two restrictions, the number of cases available for analysis is large—2,446 candidacies, all told, of which 85% involved incumbents seeking reelection.

The statistical technique we use is analysis of variance. We shall avoid discussing the technical details of this procedure and confine ourselves simply to a description of the dependent and independent variables included in the analysis, with brief comments on why and how they should be related.[10]

Consider the swing to a continuing LDP candidate in a given election year from 1960 through 1990. What might explain how well the candidate in question has done? We incorporate three explanatory variables ("main effects") in our analysis: (1) the year of the election (it might be a generally good year or bad year for the LDP as a whole), (2) the factional affiliation of

TABLE 1

Analysis of the Variance in Swings to Continuing LDP Candidates of the Top Five Factions, 1960–1990

INDEPENDENT VARIABLES	DEGREES OF FREEDOM	SUM OF SQUARES	F-VALUE	P-VALUE
FACTION	4	33.9	.55	.70
YEAR	10	4,136.8	26.65	.0001
CONCANDS	2	8,047.1	259.19	.0001
YEAR * FACTION	40	1,587.6	2.56	.0001

Note: N = 2,446; R^2 = .27. The dependent variable is the swing to continuing LDP candidate. The unit of observation is a continuing LDP candidate (from one of the top five factions) in a given year. The swing to this incumbent is defined in the usual way as "vote percentage this time" minus "vote percentage last time." The variable FACTION identifies the factional affiliation of the incumbent (coded 1–5). The variable YEAR identifies the year of the election (1960, 1963, 1967, 1969, 1972, 1976, 1979, 1980, 1983, 1986, or 1990). The variable CONCANDS equals −1 if the number of conservative candidates running in the incumbent's district decreased, 0 if it stayed the same, and 1 if it increased. The F-values given test the null hypothesis that the group means for the associated effect are equal.

the candidate in question (some factions might have done generally better or worse than others over the 1960–90 time period), and (3) whether the number of conservative candidates (including both officially endorsed LDP candidates and conservative independents) running in the candidate's district has decreased, stayed the same, or increased. (If it has decreased, one expects the swing to the incumbent in question to be particularly good, since there is one less intraparty competitor; similarly, if it has increased, one expects the incumbent's swing to be particularly poor.) The final variable that we incorporate in the analysis is a so-called interactive effect—the product of the year of election and the factional affiliation of the candidate in question. This interactive effect is important to include because the factions may have differed in the over-time pattern of their swings. For example, it may be that faction A's candidates generally lost votes in 1960 but gained them in 1963, while faction B's candidates exhibited the opposite pattern. So far as this kind of pattern exists in the data, the interactive effect between faction and year will be statistically significant.

With these expectations in mind, we can turn to the results presented in Table 1.[11] Of the three main effects, year of election and change in the number of conservative candidates are both statistically significant, but factional affiliation is insignificant. The interpretations of these three results are, (1) that the LDP as a whole (or, at least, the set of all its continuing candidates) has good years and bad—there are, in other words, detectable national partisan tides in Japan (see Kawato 1991); (2) that changing the number of conservative candidates running in a district affects the vote swings of the LDP's continuing candidates, as expected; and (3) that none of the top five factions consistently outperformed (or "underperformed") its rivals over the entire 1960–90 period. The interactive effect between year and factional affiliation is statistically significant, indicating that different factions have had different over-time patterns of electoral success (at least as far as their continuing candidates are concerned).

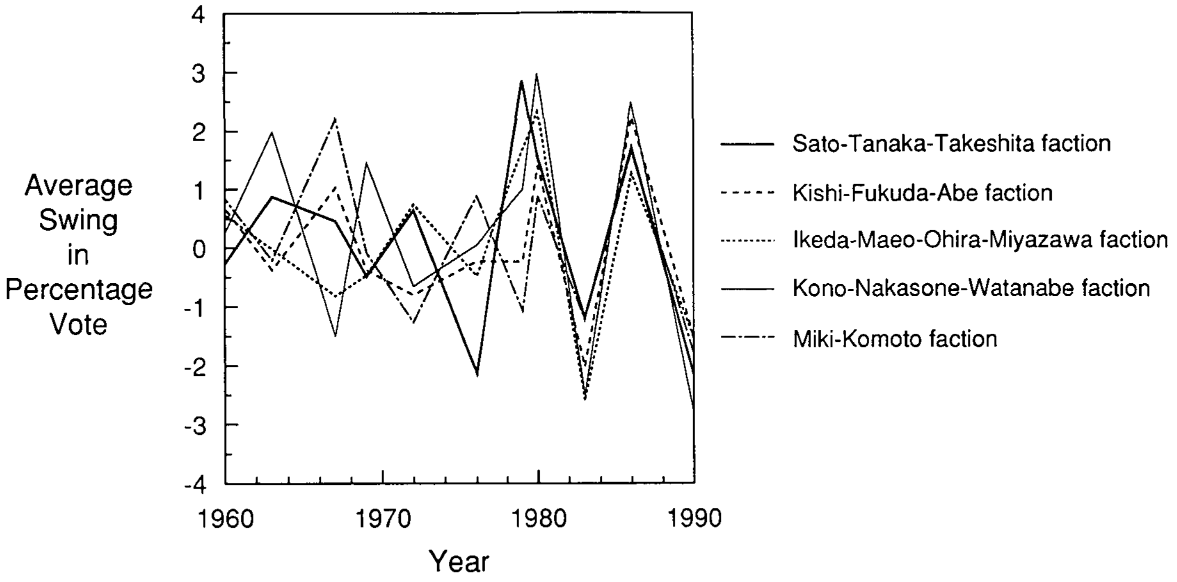
It may help in grasping this last result if the differing patterns of electoral success are presented graphically. This is done in Figure 1, which plots, for each of the top five factions, the average swing to the faction's continuing candidates against the year of election. We shall not provide a complete discussion of these graphs, but two general points are worth stressing. First, before the 1980 election, it is easy to see the differences between the various factions. For example, in five of the seven elections held from 1960 through 1979, the Sato–Tanaka–Takeshita faction and Miki–Komoto faction differed in the *sign* of their average swing, with one faction's candidates improving their vote percentages on average and the other faction's candidates doing just the opposite. Relatedly, the correlation between the swings to these two factions in this period was *negative*, as was the correlation for 6 of the 10 pairs of factions. Second, starting with the 1980 election, factional differences diminish substantially. In every election from 1960 to 1979, there were some factions with positive swings and some with negative swings. The 1980 election is the first in which all five of the top LDP factions move in the same direction, and they have done so in each succeeding election—all losing ground in 1983, gaining in 1986, and losing in 1990. Not all the factions gained and lost the same amounts, but the differences that remain do not appear large relative to those obtaining earlier (except perhaps in 1980).[12]

In order to subject this visual impression of diminished differences to a more rigorous test, we separately analyzed the swings to continuing LDP candidates in each election from 1960 to 1990. The analysis is the same as that just described except that as there is only one year involved, the main effect and interactive effect involving the year of election are removed. That is, the analysis seeks to explain the swings to LDP candidates in terms of (1) their factional affiliation and (2) whether the number of conservative candidates running in the candidate's district had decreased, stayed the same, or increased. Rather than present 11 separate tables, we merely summarize the results pertinent to the factional variable, by year (see Table 2).

As one can see, in six of the seven elections held before 1980, differences in the average swing to the factions were statistically discernible at the .10 level.

FIGURE 1

Average Swings to the Continuing Members of the Top Five LDP Factions, 1960–1990



In contrast, in only one of the four elections held in 1980, 1983, 1986, and 1990 were factional differences statistically discernible at the same level of significance. Thus, the visual impression conveyed by Figure 1 holds up to statistical analysis: the degree to which the electoral fates of the main LDP factions are tied together has increased.

In order to characterize factional differences at the polls and further corroborate their decline, we have also investigated whether mainstream factions generally did better than nonmainstream factions. The distinction between mainstream and nonmainstream factions, well known in both the literature and the popular press, is simply that between the factions supporting the winning presidential candidate, and consequently having a larger say in the running of party affairs, and those which are either neutral or supporters of losing candidates. In order to get at mainstream–nonmainstream differences, we regress V_{it}, the vote percentage for the *i*th candidate in the *t*th election, on three independent variables:

1. $V_{i,t-1}$, the candidate's vote percentage in the immediately preceding election
2. LDPSWING$_{it}$, the average swing to all continuing candidates of the LDP *other than candidate i*
3. MAIN$_{it}$, equal to 1 if candidate *i*'s faction was in the mainstream just before election *t*, 0 otherwise.[13]

One way to think of this model is as a test of whether the mainstream factions consistently outperformed the standard that one would predict from a simple "uniform party swing" model. Another way to put it is that to the extent that there are partywide forces affecting all LDP candidates, the coefficient on LDPSWING will be positive and significant; to the extent that there are mainstreamwide forces affecting all mainstream candidates and distinguishing them from nonmainstream candidates, the coefficient on MAIN will be positive and significant.

If this were a study of elections in single-member districts, the specification of the model might stop with the first three variables listed. But certain features of the Japanese electoral system require that additional controls be added. First, district magnitudes range from one to six in Japan, and obviously a candidate running in a one-seat district will have a

TABLE 2

Results of Year-by-Year Analysis of the Variance in Swings to Continuing LDP Candidates of the Top Five Factions, 1960–1990

YEAR OF ELECTION	P-VALUE FOR F-TEST NULL HYPOTHESIS
1960	.098
1963	.057
1967	.014
1969	.27
1972	.098
1976	.031
1979	.002
1983	.74
1986	.45
1990	.42

Note: The null hypothesis referred to is that the average swings to candidates of the top five factions are equal in the year in question. The P-Value gives the probability of observing as large an F-value as was actually obtained (though not reported here), were the null hypothesis true. Thus, the smaller the P-Value, the less likely the null hypothesis and the more discernible the differences among the factions.

TABLE 3

Factional Eddies within The Partisan Tide

	ESTIMATED COEFFICIENTS		
VARIABLE	1960–79	1980–90	1983–90
CONSTANT	26.33	26.08	26.59
	(1.32)	(1.45)	(1.63)
$V_{i,t-1}$	.56	.56	.54
	(.02)	(.02)	(.02)
$LDPSWING_{it}$	.86	.76	.76
	(.22)	(.06)	(.07)
$MAIN_{it}$	.77	.49	.31
	(.21)	(.24)	(.28)
MAG_{it}	−2.94	−2.63	−2.66
	(.17)	(.18)	(.21)
$NCPER_{it}$	−3.53	−4.06	−3.93
	(.31)	(.40)	(.44)
Adjusted R^2	.64	.70	.69
Number of cases	1,403	1,043	787

Note: The dependent variable is V_{it}, vote percentage garnered by the *i*th continuing LDP candidate in the *t*th election. Standard errors are in parentheses. The regression coefficients reported in the table are unstandardized.The independent variables are defined as follows: $V_{i,t-1}$ = vote percentage garnered by *i*th continuing candidate in the $(t - 1)$st election; $LDPSWING_{it}$ = average swing to all continuing candidates at election *t*, other than candidate *i*; $MAIN_{it}$ = 1 if candidate *i*'s faction was in the mainstream just before election *t*, 0 otherwise; MAG_{it} = the magnitude of the district in which candidate *i* ran at election *t*; and $NCPER_{it}$ = *the number of candidates per seat running in candidate i's* district at election *t*.

larger vote percentage than an otherwise comparable one running in a six-seat district. Second, even holding district magnitude constant, there is a wide range in the total number of candidates competing in Japanese elections; and the more candidates there are, the lower the vote percentages will be. In order to control for these two "mechanical" factors (which are almost automatically held constant in single-member, two-party systems, such as the U.S. system), we include two more variables:

4. MAG_{it}, the magnitude of the district in which the *i*th candidate runs at the *t*th election
5. $NCPER_{it}$, the number of candidates *per seat* running in the *i*th candidate's district at the *t*th election (i.e., the number of candidates divided by the district magnitude).

We ran the regression equation just specified for three periods: 1960–79, 1980–90, and 1983–90. The results, presented in Table 3, show that all coefficients (but one) are statistically significant and of the expected sign. The only coefficient that changes in a statistically significant way over the three time periods is that attached to MAIN. Whereas the coefficient is positive and significant in 1960–79, indicating consistent mainstream-specific forces at play, it is more than halved in value and no longer significant by 1983–90, indicating a substantial lessening of mainstream–nonmainstream differences at the polls.[14]

EXPECTATIONS MEET DATA

If the LDP factions differed in their electoral success largely because mainstream factions did better than nonmainstream factions, then perhaps the lessening of interfactional differences at the polls is simply a corollary to the cooling down of the mainstream–nonmainstream conflict in 1980. The story behind this cooling down is well known, so we will only give a thumbnail sketch here (see Baerwald 1986; Iseri 1988; Ishikawa 1984; Kitaoka 1990).

The 1970s were a period of intermittent "civil war" within the LDP, with hostilities peaking at the end of the decade. In 1979, for the first and last time in its history to date, the LDP failed to coordinate on a candidate for the premiership, with the mainstream (Ohira and Tanaka) fielding one candidate and the nonmainstream (Fukuda, Nakasone, and Miki) another. The mainstream candidate (Ohira) eventually won, but a few months later the nonmainstream factions absented themselves during a vote of no confidence, thus bringing down the government. Prime Minister Ohira decided to dissolve the Diet, rather than step down in favor of an intraparty opponent, so a general election ensued in June 1980. Shortly before the election, however, the factions patched up their differences, and the common wisdom in the literature is that the mainstream–nonmainstream cleavage has not been important in LDP politics since. Indeed, in 1983, Prime Minister Nakasone went so far as to declare that all factions were now mainstream (Sato and Matsuzaki 1986, 70).

The timing of the LDP civil war's end is suggestive. It is in the 1980 election, just after the war purportedly ended, that the first clear evidence of an increase in the electoral similarity of the factions is observed. Perhaps the factions decided to focus on interparty competition, rather than intraparty competition, in the interest of maintaining the (then-dwindling) LDP majority.

This story is plausible, but it does not tell us how the factions resolved their differences, what the terms of their agreement were, how they upheld the agreements they reached, or exactly how their agreements led to lessened electoral differences. Moreover, given the secrecy shrouding factional negotiations, it is unlikely that definitive answers to these questions will be forthcoming.

Nonetheless, one can speculate that any agreement intended to equalize the factions' electoral fortunes probably sought to equalize factional resources—in particular, factional access to money, posts, and/or endorsements.

Perhaps, for example, the factions agreed in 1980 to a more routinized and "fair" system of party endorsement, which led to more similar electoral fates. We shall now consider the possibility that resources other than money (esp. cabinet posts and endorsements) may have been the key to explaining why the mainstream did better than the nonmainstream before 1980. The same resources may also be the keys to explaining why the mainstream advantage lessened after 1980.

In order to deal with these alternative-resource hypotheses, we take two tacks. First, we discuss the extant evidence bearing on the allocation of cabinet posts and endorsements among the mainstream and nonmainstream factions, concluding that neither appears to have equalized anywhere near 1980. Second, we provide evidence on the electoral importance of cabinet posts and endorsements, showing that even after allowing for the impact of these resources, the mainstream factions did considerably better than the nonmainstream factions in the 1960–79 period. We then return to the discussion of money.

Allocation of Cabinet Posts and Endorsements

Most readers familiar with Japan will already know that cabinet portfolios are currently distributed among the LDP factions in proportion to their numbers in the Diet but that this was not always so (Sato and Matsuzaki 1986). Indeed, in the early 1960s, the mainstream factions consistently took a disproportionate share of posts for themselves. Does this change from nonproportional to proportional allocation of cabinet posts explain the factions' increasing electoral similarity?

The hypothesis is attractive except for the matter of timing. As Sato and Matsuzaki note, the first tentative agreement to allocate cabinet posts proportionally was concluded in Sato Eisaku's third cabinet in 1966 (1986, 67). Since 1969, the average deviation from proportional representation for the top five factions has been less than 5%. Proportional allocation of cabinet posts thus comes too soon to explain the convergence of factional electoral fates in 1980–83.

A second alternative explanation for the increasing similarity of the top five factions' electoral performances concerns endorsement policy: perhaps endorsements were handed out unequally before 1980, equally thereafter. We have addressed this question at length elsewhere (Cox and Rosenbluth 1992). We show that the mainstream factions did indeed control endorsement policy to their own benefit. In particular, mainstream factions were more successful both in getting their nonincumbents endorsed by the party and in protecting their incumbents from intraparty challengers (by denying them the party endorsement). However (and this is the important point for present purposes), the mainstream advantage does not diminish in the late 1970s or 1980s.

Those familiar with the literature may object that the mainstream–nonmainstream cleavage was no longer important after 1983. Nonetheless, we can still define the cleavage as Sato and Matsuzaki do for the 1960s and 1970s, taking as the mainstream the prime minister's faction plus those supporting him in the most recent LDP presidential contest.[15] We then find that the mainstream continues to do significantly better by endorsement policy in the 1983–90 period. In other words, the allocation of endorsements did not equalize at all and therefore cannot explain the diminishing electoral differences among the factions. If the 1980 agreement ending the LDP civil war touched on endorsement policy, it does not appear to have been successful.

Electoral Impact of Cabinet Posts and Endorsements

Even if the allocation of posts equalizes too soon and the allocation of endorsements never, it may still be that these two factors explain most of why members of the same faction move together in the electoral returns. Were this true, it would be embarrassing for the claim that money is what matters. Accordingly, it behooves us to investigate whether factional success in securing cabinet posts and endorsements does explain electoral success.

In order to address this question, we extend the regression analysis of V_{it} (the vote percentage received by the *i*th continuing candidate in the *t*th election) presented in Table 2. Table 2 contains only a single indendent variable pertinent to factional status: MAIN (equal to 1 for mainstream factions, 0 for the rest). But the coefficient of MAIN measures the net strength of *all* mainstream advantages. Thus, one cannot tell from the positive and significant coefficient displayed in Table 2 whether the mainstream advantage came in the form of posts, endorsements, yen notes, or something else.

We now add three variables that directly relate to factional success in securing posts and endorsements for their members. For posts, we employ a variable, CABINET_{it}, equal to the extent of overrepresentation on the cabinet of candidate *i*'s faction just before election *t*.[16]

Measuring factional success in controlling endorsements is more problematic. One might use an aggregate variable similar to that used for posts, for example, "the average number of nonincumbent LDP endorsees faced by members of the candidate's faction." This is unlikely to be a very revealing variable, however, since the value of having no intraparty challengers is district-specific: only the candidates from the favored district benefit, not their factional colleagues in other districts. A better way to assess the importance of endorsements, therefore, is at the district level. Accordingly, we include two variables that measure the number of nonincumbent (NONSPER_{it}) and incumbent (INCSPER_{it}) LDP endorsees per seat in candidate *i*'s district at election *t*. Because the analysis already controls for the *total* number of candidates per seat, the coefficients of NONSPER and INCSPER measure how much more damaging it is to an LDP candidate to have another candidate of his *own* party—rather than of *another* or *no* party—running. Since we have already demonstrated that the mainstream factions manipulated the number of nonincumbent endorsees to their benefit (Cox and Rosenbluth 1992), if we find that increases in this number do diminish members' vote percentages, then the

TABLE 4

Factional Characteristics and The Vote Garnered by Continuing LDP Candidates 1960–79

INDEPENDENT VARIABLE	ESTIMATED COEFFICIENTS	STANDARD ERRORS
CONSTANT	28.17	(1.4)
CABINET$_{it}$	.015	(.015)
NONSPER$_{it}$	−2.34	(.69)
INCSPER$_{it}$	−2.33	(.60)
MAIN$_{it}$	.75	(.22)
$V_{i,t-1}$	.55	(.02)
LDPSWING$_{it}$	.90	(.22)
MAG$_{it}$	−3.06	(.17)
NCPER$_{it}$	−3.23	(.32)
Adjusted R^2	.65	
Number of cases	1,403	

Notes: (1) The regression coefficients reported in the table are unstandardized. (2) The independent variables are defined as follows: (a) $V_{i,t-1}$ = vote percentage garnered by *i*th continuing candidate in the (t − 1)st election; (b) LDPSWING$_{it}$ = average swing to all continuing candidates at election *t*, other than candidate *i*; (c) MAIN^{it} = 1 if candidate *i*'s faction was in the mainstream just before election t = 0 otherwise; (d) MAG$_{it}$ = the magnitude of the district in which candidate *i* ran at election *t*; (e) NCPER$_{it}$ = the number of candidates per seat running in candidate *i*'s district at election *t*; (f) CABINET$_{it}$ = percentage of all cabinet portfolios held by candidate *i*'s faction, less the percentage of all LDP seats in the House of Representatives held by *i*'s faction, just before election *t*; (g) NONSPER$_{it}$ = number of nonincumbent *LDP* endorsees in candidate *i*'s district at election *t*, divided by MAG$_{it}$; (h) INCSPER$_{it}$ = number of incumbent *LDP* endorsees in candidate *i*'s district at election *t*, divided by MAG$_{it}$.

link between endorsement policy and electoral outcomes will have been established.[17]

In addition to the three resource-related variables just noted, and the variable measuring mainstream status, the analysis also includes the same four control variables used in Table 2: LDPSWING$_{it}$, the average swing to all continuing candidates of the LDP (other than the candidate of record); NCPER$_{it}$, the number of candidates per seat; $V_{i,t-1}$, the candidate's vote percentage in the immediately preceding election; and MAG$_{it}$, the district magnitude.[18]

Table 4 presents the results of running the regression just specified for the 1960–79 time period. We shall discuss in turn the two main resource-related variables (CABINET and NONSPER) and the mainstream status variable (the control variables behaving much as they do in Table 3).

Although the coefficient of CABINET is positive (indicating that the more overrepresented a candidate's faction was on the cabinet, the better his or her vote percentage was), it is substantively and statistically insignificant. This lack of significance might of course change if we weighted portfolios somehow to indicate electoral importance or included other posts in the reckoning, for example, those in the Policy Affairs Research Council (PARC). For now, however, evidence is lacking that a disproportionate share of cabinet posts meant significantly more votes in this period.[19]

The coefficient of NONSPER reveals that one more nonincumbent endorsee in a three-, four-, or five-seat district on average diminished the vote percentages of the continuing candidates in those districts by 1.86, 1.39, and 1.11 percentage points, respectively.[20] These percentages are fairly hefty when viewed as a proportion of the total percentage usually garnered by winning candidates. For example, a typical winning percentage in five-seat districts is 15%; compared to this, a loss of 1.11% looks rather large.

Thus, given that the mainstream factions have consistently manipulated endorsement policy to their own benefit (see Cox and Rosenbluth 1992), one can conclude that control of endorsement policy has contributed to the mainstream's electoral superiority. Nonetheless, comparing the size of the coefficient estimate for MAIN in Tables 3 and 4 suggests that endorsement policy does not explain much of the mainstream advantage. When endorsement policy is not controlled (Table 3), the coefficient estimate for MAIN is .77 (other things being equal, a mainstream candidate got almost eight-tenths of a percentage point more than an otherwise comparable nonmainstream colleague). When endorsement policy is controlled (Table 4), the coefficient estimate for MAIN falls only to .75, indicating that something else must explain the vast bulk of the mainstream advantage.[21] We think that the only plausible remaining advantage of sufficient importance to produce a coefficient of the size observed is money.

Where do these findings leave us? The allocation of cabinet posts among the top five factions has been largely proportional from 1969 on, and the small differences that remain do not explain the continuing differences in electoral success. The allocation of endorsements among the top five factions has been consistently biased in favor of the mainstream over the entire time period investigated and in any event, after statistically controlling for its advantage in endorsement policy, the vast bulk of the mainstream's advantage in 1960–79 remains. In light of these findings, it is difficult to argue that cabinet posts or endorsements are the key to the factions' electoral convergence. This leaves money (an equalization of factional war chests) and noncabinet posts (an extension of the norm of proportionality to other important posts, such as those in the PARC) as the most plausible explanations.

We have of course already argued that the decentralization of fund-raising should have equalized factional war chests at roughly the time that one observes electoral convergence. Nonetheless, because it is impossible to discover the amount of money each member spent or the portion of his funds coming from the faction, one cannot directly verify either the electoral importance of money or the exact timing of the decentralization in fund-raising. It would be helpful, therefore, to marshal some additional indirect evidence.

We mention three kinds of such evidence here.

First, the decentralization hypothesis (together with an ancillary assumption about how factional bosses distributed their money) implies a boost in the variance of funds raised within a given faction over time. For if bosses give roughly equally to all members (or more to the more needy),[22] then boss-dominated fund-raising will lead to rough equality of funds within the faction. In contrast, as the factional contribution shrinks as a proportion of total funds, each member's fund-raising ability more and more determines the size of his war chest, so that intrafactional inequalities in funds should increase. To the extent that intrafactional inequalities in funds lead to intrafactional disparities in electoral performance, this means that one should see a larger standard deviation of swing within each faction.

As it turns out, this expectation is borne out by the data. We have looked at the standard deviation of the swings to incumbents in each faction over time and found (pooling all factions) that there is a statistically discernible increase. The problem with this evidence is that other processes can account for it as well. For example, the four largest factions grew substantially in the late 1970s and early 1980s. Such growth meant that factional members ran in a larger and more diverse set of constituencies, which by itself may have produced a mild increase in the variance of swings within factions.

A second bit of indirect evidence in favor of the argument put forward here concerns factional exposure to scandal. If fund-raising has decentralized, then the likelihood of any one faction monopolizing funding sources should have declined. In particular, it should be less likely that any faction is entirely "clean" when it comes to taking money illegally. Thus, on the hypothesis of decentralization, one expects what has been observed in the latest round of scandals (Recruit, Kyowa, and Sagawa), namely, that no faction is well positioned to pose as "Mr. Clean" and thereby benefit at the expense of its factional competitors (as the Miki faction was in regard to the Lockheed scandal).

The increasing role of the PARC provides a final bit of evidence that fund-raising did decentralize. As the conventional wisdom in the literature has it, PARC positions have grown in importance since the early-to-mid 1970s. This fits nicely with a decentralization-of-fund-raising story. The enhancement of PARC positions was tantamount to a decentralization of power within the LDP's Diet contingent, from frontbenchers to backbenchers. This decentralization of power, in turn, gave more members a base from which to attract contributions. Thus fluctuations in the interfactional allocation of PARC posts might have materially affected factions' fund-raising success and hence their electoral success. In particular, supposing posts were inequitably divided among the factions before about 1980 but equitably (i.e., proportionally) divided thereafter, one has another possible wrinkle in the fund-raising story. From this perspective, it is intriguing to note that the most unhappy nonmainstream factions in 1979 (Fukuda and Miki) were substantially underrepresented in PARC leadership positions while the mainstream factions (especially Tanaka) were overrepresented. By September 1980 (after the end of the civil war), Fukuda had made large gains, mostly at Tanaka's and Ohira's expense.[23] If these changes reflect not just Ohira's desperate attempts to hold on to the premiership but instead a conscious extension of the proportionality norm to PARC leadership positions, then they gibe with our story. It remains to be seen, of course, whether a longer time series of observations on the allocation of PARC posts among the factions supports such an interpretation. We plan in future to compile the necessary data on appointments to address this question; we can then analyze the electoral impact of PARC posts along the same lines taken here for cabinet posts.

CONCLUSION

We have investigated a topic not often explored in the literature on Japanese elections—the degree to which the top five factions of the dominant LDP have differed in their electoral success. Our study has produced three main empirical findings. First, the electoral success of top five factions differed noticeably in the 1960s and 1970s. Although no faction consistently outperformed (or lagged behind) its rivals, overtime patterns of success differed markedly. Second, the mainstream factions did consistently better than their nonmainstream rivals prior to 1980. Third, during the 1980s, electoral differences among factions have lessened dramatically.

We explain all three facts (that the factions once differed, that they differed along mainstream and nonmainstream lines, and that interfactional electoral differences have greatly diminished) by reference to fund-raising. In particular, we rely on the hypothesis that fund-raising has decentralized. The gist of our explanation is as follows. Money is an essential ingredient in a successful campaign for the Diet. When fund-raising is a factional affair, with each boss raising most of the money for his followers, factional receipts can vary more widely (on a per capita basis) than when fund-raising is highly decentralized, with every member raising most of his own money. The reason for this, embodied in the statistical "law of averages," is essentially that the mistakes of a single boss are not counterbalanced by successes elsewhere, as happens when fund-raising is decentralized. Thus, to the extent that differences in the size of factional war chests drive differences in electoral success, one expects a decline in the electoral distinctiveness of the factions as fund-raising decentralizes.

This view of the declining electoral distinctiveness of the LDP factions, although it has much going for it, also leaves some important questions unanswered. First, the process of decentralization has gone on largely unobserved and unmeasured. Did the bulk of decentralization really occur just before the observed electoral convergence in 1980 and 1983? Second, other Japanese legislative parties are also factionalized to varying degrees. But according to standard accounts of Japanese politics, non-LDP factions are not as active as LDP factions in raising and distributing campaign funds.[24] Insofar as this standard characterization of non-LDP factions is accurate, the decentralization hypothesis should not apply in their case. A pattern of declining electoral distinctiveness among non-LDP factions, such as we noted among LDP factions, would imply a more general explanation than one based on changing fund-raising practices. Is there such a pattern?

As regards the first of these questions, we shall simply note that if we are right, and the lessening of interfactional differences stems primarily from the decentralization of fund-raising, then the disappearance will be *permanent*. If interfactional differences reemerge at the next election, it will be hard to maintain that the decentralization of fund-raising explains the 1980s (unless, of course, there is evidence that fund-raising has recentralized).

As regards the second question, we hope in future to investigate the electoral fortunes of factions in the Japan Socialist party in the same way that we have investigated LDP factions in this paper. If the Socialist party factions do in fact compete along ideological lines rather than with the distribution of tangible benefits, there is no reason to expect their electoral fortunes to have aligned in the way that the LDP's factions' fortunes have.

Notes

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1. An early and influential cultural explanation of LDP factionalism is that of Nakane (1967). See also Doi 1971; Thayer 1969; and Yamamoto 1989.

2. Kyogoku (1983) also emphasizes the electoral system. He, however, takes a different tack than do Okimoto or Fukui, stressing not the necessity of intraparty competition but the ability of small groups to secure representation: "The medium-sized electoral district system, because it advantages representatives of small groups, produces both multiple parties and factions within the LDP" (p. 40).

3. Factionalism within the LDP thus bears a striking similarity to that in the Democratic party in the U.S. South. In both cases, a dominant party's choice of nominee was tantamount to the broader political system's choice of chief executive; and in both cases, the dominant party was factionalized (see Key 1949).

4. Obviously, there is some difficulty in deciding who has replaced whom when incumbents of more than one party lose in a single district. As it turns out, in the vast majority of districts in which an incumbent LDP candidate loses, only LDP incumbents lose, so it is easy to determine who has replaced them. In districts where both LDP and other incumbents lose, we have assumed that each new winner was equally likely to replace any of the defeated incumbents.

5. On 16 May 1992, a group calling themselves the Young Dietmembers' Club released their financial records, broken down by sources of income and types of expenditures. While helpful, the only comparable figures are those released by a similar group, the Utopia Club, in 1989. Piecing together comparable figures for earlier periods involves much guesswork. The released figures also provide us with no clue as to how fund-raising or spending breaks down by faction.

6. Thus one expects factions with highly centralized fund-raising to be more frequently distinct in their degree of success than factions with highly decentralized fund-raising for essentially the same reason that one expects single observations on a random variable to fluctuate more widely than do averages of observations.

7. We refer to the decentralization hypothesis rather than to the decentralization fact because, although there is substantial evidence that fund-raising has decentralized (see Iwai 1990; Kohno 1992), most of it stems either from interviews with politicians (who are not always the most reliable sources about either their own or others' finances) or from manipulations of the officially reported data on campaign expenditures (which are woefully inadequate). Our approach is not to look for direct evidence that decentralization has occurred but to test an implication of decentralization in light of the argument just presented. The best figures on the percentages of Diet members' funds coming from the faction at various points in time are as follows. Totten and Kawakami (1965) estimate that factional leaders supplied candidates with approximately a third of their campaign war chests in the 1960s. Kawanishi (1991) suggests that factional leaders raised only 2–3% of total campaign money after the Recruit scandal. A 16 May 1992 press release of campaign figures by the Young Dietmembers' Club suggests a figure in the 7–8% range (see also Iwai 1990).

8. Restricting the data in this way does introduce a sample selection problem: those who feel sure they are going to lose may decide not to run again and hence will not appear in our data. This does not appear to be a serious problem for the conclusions we wish to draw, however.

9. We confine attention to the top five factions in part because they are the most important and in part because including all factions poses some additional (mild) technical difficulties that we wish to avoid at this point. We intend to include all factions (indeed, to focus on the smaller ones) in future research.

10. An introduction to analysis of variance can be found in almost any introductory text on statistics. See, e.g., Agresti and Finlay 1986, 50–52, 58.

11. The data used in this and all other analyses conducted in this paper are from Steven Reed's (n.d.) forthcoming compendium, *Japanese Election Data: The House of Representatives 1947–1990*.

12. A third interesting feature of the graphs presented in Figure 1 is that the *size* of the average swings to each faction's candidates appears to grow. If one pools all continuing LDP candidates incumbents together, one finds that the average absolute swing is about one-percentage-point larger after 1980 than before (the difference being statistically significant at the .01 level). The only faction for which this pattern does not appear to hold is the Miki–Komoto faction.

13. We followed Sato and Matsuzaki (1986) in identifying mainstream status down until 1983, when their coverage stops. In three instances, their defining criterion of mainstream status (having supported the winning candidate in the most recent LDP presidential election) was not fully satisfactory for our purposes. In each of these instances, there appeared to have been some shifts in the intraparty alliance structure between the LDP presidential and general elections. When we recoded the mainstream status of the factions involved, however, virtually no change occurred in the coefficient estimate of MAIN. For 1986 and 1990, we used newspaper reports in *Asahi* and *Nikkei* to identify the mainstream. For 1986, we took the mainstream to be the Takeshita and Nakasone factions. For 1990, we coded the mainstream as all factions except for that headed by Miyazawa. If we take more restrictive views of what the mainstream was in 1990 (either

Takeshita–Abe–Komoto or Takeshita–Komoto), our results remain substantially the same.

14. If one estimates three separate regressions for the three-, four- and five-seat districts respectively, one gets a similar coefficient estimate for MAIN in each case, although it does decline slightly with district magnitude, as would be expected.

15. See n. 13.

16. That is, CABINET equals the percentage of all cabinet portfolios held by candidate *i*'s faction, less the percentage of all LDP seats in the Diet held by candidate *i*'s faction, just before election *t*.

17. Including a variable for the number of conservative independent candidates per seat alters the results hardly at all, although it does show that these candidates take more votes away from LDP candidates than do those of opposition parties.

18. We have also run the regression with a time trend. The trend is significant and negative, as one would expect, but as including it does not change the coefficient estimates of the other variables, we have omitted it from an already-cluttered analysis.

19. The lack of significance persists if one uses simply each faction's share of cabinet portfolios as the independent variable. The primary reason for the lack of significance is simple: from 1969 on, the top five factions' shares of cabinet posts were very nearly proportional to their respective sizes, so there is very little variance in the independent variable after 1969 to explain anything. Even before 1969, the relationship is not strong.

20. Recall that NONSPER measures the number of nonincumbent endorsees per seat. This choice of unit means that if one wishes to discover how much one more nonincumbent endorsee cost the typical continuing LDP candidate, then one must first divide the coefficient estimate by the magnitude of the district in which the candidate ran. This division is necessary because "one more nonincumbent endorsee" means "1/3 more nonincumbent endorsees per seat" in a three-seat district, "1/4 more nonincumbent endorsees per seat" in a four-seat district, and so on. It should also be noted that "one more nonincumbent endorsee" counts once as a "candidate" (in NCPER) and once as a "nonincumbent endorsee" (in NONSPER), so the total effect is found by adding the coefficients of NCPER and NONSPER, then dividing by district magnitude.

21. The mainstream advantage declines so little when one controls for endorsement policy because the "extra" endorsements the mainstream gets are often given to first-time candidates, who are not in the data set of continuing candidates. Thus although mainstream manipulation of endorsement policy gives little help to the mainstream's continuing candidates (most of whom are incumbents and automatically endorsed), it may be quite consequential for the mainstream's new candidates.

22. Support for these suppositions can be found in Thayer 1969, 30.

23. By "leadership positions" we mean chairs and vice-chairs. The measure of overrepresentation is simply a faction's percentage of PARC leadership positions less its percentage of LDP seats in the House of Representatives. In 1979, Tanaka and Ohira were overrepresented by 9.3% and 4.0%, respectively, while Fukuda and Miki were underrepresented (−9.0% and −9.4%). By September 1980, Fukuda was overrepresented (8.3%), and Tanaka and Ohira underrepresented (−.7% and −4.9%). Fukuda had already made gains, and Tanaka and Ohira losses, before the end of the civil war.

24. See, e.g., Masumi 1985, vol. 2, p. 489–564.

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TOLERATING ECONOMIC REFORM: POPULAR SUPPORT FOR TRANSITION TO A FREE MARKET IN THE FORMER SOVIET UNION

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The mass public in the Soviet Union is not enthusiastic about free-market reform. How, then, do citizens in a former communist regime develop an appreciation for free-market reforms? Different explanations for attitudes toward free market reforms are tested using data from a survey of the European USSR conducted in May 1990. First, negative assessments of recent economic performance is a catalyst for popular support for the market economy. Although very underdeveloped, there is a nascent free-market culture in the Soviet Union that makes a modest contribution to support for free-market reforms. The free-market culture that is developing in the former Soviet Union resembles that of social democracy, rather than laissez-faire capitalism. Democratic values and support for free markets are mutually reinforcing, suggesting that support for democracy makes a very important contribution to support for free-market reform.

The Soviet Union and the countries of Eastern Europe have embarked upon an experiment that reverses the historical relationship between capitalism and democracy.[1] Most countries experienced democratization after the introduction of free-market institutions (e.g., the recognition of certain basic property rights, enforcement of contracts, a free-market pricing system). Having adopted democratic institutions, the successful implementation of free-market policies will be influenced by the willingness of the Soviet citizens to accept what are often rather draconian measures with very serious short-term costs, such as unemployment and rapidly rising prices.[2] But why would citizens support policies that have negative affects on the national economy and on their own standard of living? In order to answer this question, I address what Albert Hirschman (1981) characterized as the political economy of patience.[3] I shall explore the factors that shape the response of the Soviet mass public to free-market reforms: Why are some citizens patient, while others are not?

Armed with their recently acquired democratic rights, the mass publics in the former communist regimes are registering their growing *impatience* with free-market reforms (Bruszt and Simon 1991). The fall 1991 parliamentary elections in Poland registered the mounting popular dissatisfaction with free-market reforms. Even in the former German Democratic Republic, popular support for market reforms dwindled as the hardships mounted. Responding to this dilemma of rising democratization and growing frustration with free-market reforms, some political leaders are trying to insulate themselves from democratic influences. As part of his plan to introduce economic reforms, Russia's president Yeltsin requested greater executive power and asked for the postponement of local elections. Polish president Lech Walesa has also repeatedly asked for increased presidential power in an attempt to insulate his market reforms from legislative oversight. Others, such as the leaders of the August 1991 coup in the Soviet Union, have tried to exploit popular antagonism to market reform for antidemocratic goals.

Can these countries both maintain democratic institutions and weather the transition to a free-market economy? An important contributing factor is public support for these initiatives, my central focus here. First, Soviet citizens are hypothesized to respond negatively to mismanagement of the overall economy by embracing alternative economic solutions. Over the past 20 years the Soviet economic situation has deteriorated, culminating in economic chaos in 1990. Given the historical failure of central planning, free-market reform is hypothesized to represent an alternative policy choice for the average citizen. This support will be forthcoming even though individual citizens in 1990 could expect serious economic dislocation as a result of these reforms: they could see the serious hardships faced by other former communist regimes that had already undertaken the transition, in particular, Hungary and Poland (see Gelb and Gray 1991). Because Gorbachev had initiated so few fundamental economic changes by 1990 there is little question that central planning was the likely scapegoat for the country's economic chaos (Goldman 1991).

But if assessments of economic performance were the sole factor driving support for free-market reform, the prognosis for their popular acceptance would be poor. Citizens are more likely to tolerate the social dislocation of economic reform if their support for change is built upon more substantial considerations than simple responses to economic fluctuations. I hypothesize that preferences for free-market reform reflect an acceptance on the part of individuals of certain basic premises of free-market mechanisms—what I call a *free market culture*. But given the collectivist culture of both Soviet and prerevolutionary Russia, this could represent a narrow foundation.

A more promising candidate is support for democracy. Scholars have long argued that there is a strong correlation, at least at the institutional level, between democracy and free markets (Dahl 1989; Hayek 1944). This raises the possibility that at the individual level preferences for free-market reforms are likely to follow from the fairly widespread diffusion of democratic norms in the Soviet Union (Duch and Gibson 1991; Gibson and Duch 1993).

Recognizing that free-market reforms almost inevitably result in widespread short-term economic dislocations, political officials fear that support for such initiatives can easily dissipate.[4] This pessimism has been echoed by many scholars, who argue that there is an "irreconcilable antagonism between democracy and property and price reforms," primarily because of the "consequences of both of these economic reforms (namely, unemployment and inflation)."[5] If support for free-market reforms is driven by short-term responses to existing economic conditions, their fears are well grounded. Alternatively, if support for free-market reforms is built upon more fundamental foundations (e.g., a free-market culture and democratic values), the prospects for such policy initiatives are much brighter.

I shall test four explanations for how Soviet citizens respond to free-market reform: (1) individuals' perceptions of economic performance, (2) socioeconomic structural self-interest, (3) the presence of a nascent free-market culture, and (4) support for democratic norms. The data are from a survey of the European USSR mass public conducted in May 1990. I begin by reviewing the explanations for support for free-market reform, present the operationalization of both dependent and independent variables, follow with a discussion of the univariate results, and end with a presentation of the multivariate findings.

THEORIES OF SUPPORT FOR ECONOMIC REFORM

Two important factors shape individuals' policy choices. First, it is assumed that citizens make instrumental decisions regarding economic policies based on their assessment of past and future conditions. Those who link deteriorating economic conditions with past policies of the command economy will embrace new market reforms, while the contented will reject them. Additionally, those who expect to benefit directly from economic policy changes will support them. Second, cultural predispositions constrain the range of policy options that citizens will consider legitimate. Two such constraints are explored here: (1) the popular diffusion of a free-market culture and (2) the degree to which citizens support democratic reform. I begin with a discussion of the instrumental calculations (evaluations of the economy and of economic self-interest) and then move to a discussion of the two cultural constraints.

Economic Conditions and Support for Market Reforms

There is widespread agreement that deteriorating economic conditions contributed to both elite and mass support for political and economic change in the Soviet Union.[6] The interesting paradox is that the policies that Soviet elites are forced to adopt will, at least in the short term, make the economic situation worse. In Eastern Europe support for rapid liberalization deteriorated rather dramatically once the costs of these initiatives became evident, which posed serious political problems for the new governors (Wolf 1991). This tendency complicates efforts to model the impact of perceptions of the economy on mass policy preferences, because there is considerable uncertainty as to who the average individual blames for the prevailing economic situation. Let us first explore the critical aspects of the relationship between perceptions of the state of the economy and political preferences.

Extensive evidence from the developed democracies suggests that political evaluations are shaped primarily by evaluations of overall economic performance, what are referred to as sociotropic assessments. Citizens tend to be motivated by their assessment of the performance of the collective economy (e.g., overall business conditions), rather than personal or "pocketbook" concerns (Lewis-Beck 1988, chap. 3). These studies testify to the importance of both retrospective evaluations of the overall economy (Hibbs 1982; Kiewiet and Rivers 1984; Kinder and Kiewiet 1979; Lewis-Beck 1988) and prospective, or future, evaluations of economic conditions (Chappell and Keech 1985; Fiorina 1981; MacKuen, Erikson, and Stimson 1992).

The retrospective model of economic attitudes suggests that dissatisfaction with status quo policies will generate support for changing either the incumbent or the existing policies. In this case, status quo economic policies are represented by the command economy put in place by the Soviet communist regime. Under Gorbachev, the basic central-planning paradigm was maintained, although he did establish some of the preconditions to a market economy (Dowlah 1992, 179; Goldman 1991). Therefore, I hypothesize a negative relationship between retrospective assessments of the economy and support for free-market reform, the alternative to the existing command economy.[7]

Prospective evaluations of the overall economy are hypothesized to have exactly the same impact as did retrospective assessments. Soviets who expected the overall economy to deteriorate are predicted to support free-market changes. The reasoning here is that the average citizen, even given the limited economic reforms of Gorbachev, continued to associate future macroeconomic problems with the command economy and therefore embraced the free market as offering a solution.[8] Hence I expect prospective assessments of the overall economy to be negatively related to acceptance of free-market initiatives, reflecting the rejection of the communist's command

TABLE 1

Economic Assessments and Support for Perestroika, European USSR, 1990 (%)

EVALUATION OF PERESTROIKA[c]	CONDITION OF ECONOMY TODAY[a]			ECONOMIC SITUATION NEXT YEAR[b]		
	GOOD	AVERAGE	BAD	IMPROVE	SAME	WORSEN
Has not begun	11.8	9.6	18.7	5.5	12.3	24.6
Has stopped	5.9	6.5	16.9	4.7	14.1	19.1
Major difficulties	76.5	66.8	58.4	75.4	63.5	51.0
Some difficulties	5.9	16.9	5.9	14.0	10.0	5.3
Developing with no problems	.0	.3	.1	.4	.1	.0
Total	100	100	100	100	100	100
Number of cases	17	355	1,083	236	690	545

[a] The question reads, "How would you rate the condition of the economy in our country today: good, average, or bad?

[b] The question reads: "And what do you think will happen in our economy in a year? Do you think the economic situation in the country will improve, worsen, or will nothing change?

[c] The question reads, "How do you think *perestroika* is getting along in the country?

economy. A positive relationship between the two variables would clearly challenge this reasoning. Such a relationship would indicate that as early as 1990 those expecting the overall economy to deteriorate had become the foes of market reform.

A critical assumption here is that citizens embrace the market economy because they are dissatisfied with economic conditions that they link to the centrally planned economy of the communist regime. It is assumed that in 1990 citizens attributed responsibility for both past and anticipated economic problems with the old regime. This assumption is complicated by the fact that Gorbachev had initiated very limited reforms by 1990, when the survey was conducted. Hence there is some possibility that a significant sector of the population in 1990 linked economic deterioration to free-market initiatives. The cross tabulations in Table 1 suggest this was not the case. Respondents were given an opportunity to evaluate the policies of perestroika.[9] Two of the options were that perestroika had not even begun and that the implementation of perestroika had stopped. Both options suggest an impatience with the slow progress toward a freer economy. If the economically disgruntled were blaming free-market policies, then they would be the least likely to express impatience with the pace of perestroika's reforms. In fact, for the two questions regarding the overall economy, the disgruntled are the *most* likely to indicate that perestroika had stalled. For example, 25% of those who expected the economic situation to worsen the following year thought that perestroika had not even begun, while among those expecting improvement in the economy only 6% shared this opinion. To conclude, those who think the overall economy has performed—or will perform—poorly are more likely to be unhappy with the slow pace with which perestroika has been implemented. The economically disgruntled do not seem to be blaming free-market reforms.

Political preferences can also be shaped by how individuals think the economy affects their personal financial well-being, ("pocketbook" considerations), as opposed to evaluations of the overall collective performance of the economy. But the evidence suggesting that sociotropic motivations outweigh pocketbook explanations is considerable (Lewis-Beck 1988; Kinder and Kiewiet 1979; MacKuen, Erickson, and Stimson 1992), and therefore I do not expect that they will play a particularly important role in this context.[10] Nevertheless, to the extent that there is a pocketbook effect, I hypothesize a negative relationship between it and support for market reform. The more one expects one's personal economic situation to deteriorate, the more likely one is to embrace these economic changes, because (as was just demonstrated) responsibility for pocketbook conditions is attributed to the previous regime. Once again, I expect these pocketbook concerns to be considerably overshadowed by preoccupations with the overall economy.

Let us briefly summarize these hypothesized relationships. Retrospective assessments of economic performance are hypothesized to be negatively related to support for economic reform: negative evaluations should generate a rejection of past economic policies and support for change. Similarly, I expect that the relationship between prospective evaluations of economic conditions and support for free-market change will be negative because at the time citizens still associated future economic problems with the past regime. Thus, the strongest proponents of market reform are those who see the overall economy deteriorating.

Socioeconomic Self-Interest

Zaslavskaya points out that economic reform in the Soviet Union offers important long-term benefits for certain sectors of society while making life more difficult for other segments (1990a, 157). This argument suggests that above and beyond the short-term costs associated with economic reform, individuals can clearly recognize how structural changes in the economy will affect their long-term interests. For example, Zaslavskaya also notes that the average skilled worker will support economic reform because skilled workers have "both the possibilities and the vital need for extra earnings" (p. 164). On the other hand, skilled workers in the distribution, public catering, and personal and domestic service sectors are likely to oppose free-market reforms, because these workers derive significant material benefits from access to goods and services that were virtually unavailable to the typical consumers. Access to these privileges assured them living standards higher than their nominal salaries would indicate. Hence those benefiting from these privileges might be expected to oppose privatization because it would likely eliminate these special benefits.[11]

But an average citizen's ability to evaluate the long-term implications of the free market assumes sufficient information to make such calculations. Given that the market economy represents such a sudden break with Soviet traditions and that prior to perestroika it received only limited (and generally negative) public discussion, individuals may have had insufficient knowledge to assess how the market economy would affect their personal economic interests.[12] Limited information might undermine the correlation between self-interest and free-market preferences.

Although detailed information on the objective situation of different occupational groups in the former Soviet Union was not available, I can test two rather broad generalizations about the self-interest of groups in Soviet society. First, I propose a crude ranking of free-market self-interest. Following Zaslavskaya (1990a), I expect those working in industry and agriculture to be the most enthusiastic about free-market reforms. Again following Zaslavskaya, I expect the service sector of the Soviet economy will be less enthusiastic about reforms because it has benefitted most from "informal" access to a variety of difficult-to-obtain goods and services. All of these sectors should be more supportive than those on state aid, the majority of which consists of pensioners.

Other socioeconomic groups are likely to be affected in a systematic manner by economic reform, thus creating socioeconomic groups of winners and losers. First, I hypothesize that the younger and better-educated in Soviet society are more likely to support free-market reforms, partially out of a recognition that they would be better equipped to succeed professionally and financially in such an environment. Second, I expect the wealthy to be less supportive of economic changes because free-market reforms would threaten their privileged access to goods and services. There is little reason to expect the beneficiaries of the economic status quo to be strong advocates for change.

Thus I expect socioeconomic structural self-interest to play an important role in shaping preferences, even though information costs and measurement problems might somewhat undermine the relationship.

Free-Market Values

Market reforms result in very serious short-term dislocations and hardships (International Monetary Fund et al. 1991). Therefore, to the extent that support for market reforms is built on a foundation of fundamental beliefs, Soviet citizens are more likely to support these institutional changes even in the face of serious hardships. One of these fundamental values is a free-market belief system. These values are distinct from preferences for institutional reform. The latter concern support for specific policy initiatives such as privatization whereas free-market values are fundamental beliefs (e.g., how market mechanisms operate and what levels of inequality are appropriate in society). Although the two are correlated, there is no reason to consider them synonymous. It is conceivable that individuals in many capitalist nations support the link between incentives and economic efficiencies without necessarily supporting the privatization of state-owned firms. There are societies (e.g., Sweden) where citizens have expressed a preference for significant reductions in wealth and income inequalities but at the same time express strong support for market pricing mechanisms. Thus, I contend that a free-market belief system stimulates—but is hardly synonymous with—support for free-market reforms.

If support for free-market reform is predicated on support for free-market principles, the success of market reforms would therefore be somewhat contingent upon how widely these principles (e.g., the role of inequality as an incentive in the market economy) are embraced. Recent survey evidence from the Soviet Union gives reason for some optimism. A comparison of Soviet and U.S. beliefs about free markets concluded, rather surprisingly, that the two mass publics have very similar attitudes towards fairness, income inequality, and incentives and in their understanding of the working of markets (Shiller, Boycko, and Korobov 1991).

But there is a strong argument to the contrary suggesting that fundamental beliefs about the free market have been shaped by centuries of official norms hostile to the institution. According to many scholars, Soviet cultural traditions undermined the development of a free-market culture (Pipes 1990; H. Smith 1989; White 1979). For example, they point to the collectivist traditions of the old Russian mir dating back to Czarist Russia that emphasized common ownership of land and disapproved of individual enterprise. This, of course, was followed by the collectivist traditions of the communist regimes. Social-learning theories would argue that official propaganda has undermined any support for the free-market among the mass public (Gibson and Duch 1993).[13] Several Soviet scholars have endorsed this argument that the Soviet mass public has embraced official norms (e.g., DiFranceisco and Gitelman 1984; White 1977, 1979). Moreover, much of the internal debate over economic reform in the Soviet Union has emphasized the obstacles that Soviet economic culture poses for the implementation of free-market reforms (Shmelyov 1991).

Thus, I am hypothesizing a strong *relationship* between free-market culture and support for free-market reforms. But *levels* of commitment to free-market beliefs will be low given historical traditions in the Soviet Union.

Democratic Reform

The popular endorsement of democratic institutions represents a potential foundation for free-market reforms. There is a rich philosophical literature that emphasizes the link between democracy and the free market.[14] A society that has embraced the notions of liberty and the competition of ideas—so integral to democracy—is therefore likely to support the concepts of liberty, freedom, and competition that are critical to free markets. Hence, those who have embraced democratic norms are expected to respond positively to free-market changes. Given relatively high levels of support for democracy in the Soviet Union (Gibson and Duch 1993), this suggests that large proportions of the population should embrace free-market reforms.

But the historical record questions the importance of democracy as a foundation for free-market reform. History suggests a strong correlation between the two institutional reforms but at least in the past, free-market economies have *preceded* the emergence of democratic institutions. Many nondemocratic societies have accepted free-market institutions, but all democratic societies have had free-market economies (Berger 1986; Dahl 1989; Lindblom 1977, chap. 12).[15] The Western European model of development is one that starts with the nation–state and moves to capitalism and finally to democracy. At least at the level of national institutions, free markets have been a necessary—but not a sufficient—condition for the development of democratic institutions.[16] At the individual level, this suggests that the development of free-market preferences might predate the acquisition of democratic values, but not vice versa. To some extent the authoritarian model of capitalism (along the lines of Pinochet in Chile and Park in South Korea) presumes that a well-developed democratic culture is not essential to the introduction of free-market institutions.

But recent transitions to democracy and free markets in the former communist regimes occurred under different historical circumstances. In the case of transition from communist regimes, support for democracy is probably more widespread than for free markets, raising the possibility that democratic norms fostered support for economic change. One reason is that Soviet citizens, particularly since the early 1960s, had some limited opportunities for democratic participation and therefore the development of democratic norms. In fact, there is evidence that participation of a democratic nature was promoted by the totalitarian regime and by official ideology (see Hahn 1988; Hough 1976). Recent survey research from the Soviet Union seems to confirm this fact (Bahry and Silver 1990; Duch and Gibson 1991; Zimmerman 1987). Free markets and capitalism, on the other hand, were not tolerated either in practice or according to official ideology. (Even the exceptional concessions to markets such as private plots in agriculture were allowed very grudgingly.) This raises the possibility that individuals were much more likely to develop an appreciation for democracy than for capitalism. Hence, the transition from communism to democratic capitalism may represent one of the unique situations where democracy was the catalyst for free-market reform. Thus I expect greater levels of support for democratic institutions as compared to free-market reform.

Establishing that preferences for democracy are causally prior to support for free-market reforms will be difficult, given the data available. It may in fact be the case that individuals developed these preferences for democratic institutions prior to embracing the market economy but (at least at any point in time) the same set of factors are likely to correlate with both variables, making it difficult to identify any nonrecursive model that included support for democracy and for free-market institutions. Nevertheless, it is reasonable to expect a strong correlation between support for democracy and for the market economy.

To summarize briefly, I have hypothesized that four factors shape preferences for market reforms: economic performance, self-interested structural opportunities generated by these changes, diffusion of a free-market culture, and popular endorsement of democratic institutions.

SUPPORT FOR MARKET REFORM IN THE SOVIET UNION

These arguments regarding the development of support for free-market reforms are evaluated on the basis of a representative sample of Soviet citizens 16-years-old or older residing in the European portion of the USSR. Interviews were conducted in May 1990. The face-to-face survey of approximately fifteen hundred Soviet citizens was conducted in association with the Institute of Sociology, USSR Academy of Sciences.[17]

I begin by defining popular support for free-market policies, the dependent variable in this analysis. Three dimensions of the free-market stand out as particularly important for the general public: liberalization of prices, a free labor market, and private ownership.[18]

Most economists consider the liberalization of prices to be an essential element of market reform in the Soviet Union and Eastern Europe. Moreover, it is a centerpiece of Yeltsin's economic reforms (International Monetary Fund et al. 1991; Kornai 1990, 146; Lipton and Sachs 1990).[19] But efforts to introduce radical price reform have been resisted in the Soviet Union. For example, Gorbachev rejected the Shatalin plan and Yeltsin hesitated regarding freeing energy

TABLE 2

General Support for Economic Reform, European USSR, 1990

ITEM	SUPPORT MARKET REFORM	CAN'T SAY EXACTLY	OPPOSE MARKET REFORM	TOTAL	FACTOR LOADING
Radical economic reform is necessary[a]	54.3	20.3	25.4	100	.54
Favor an economy in which prices set by supply and demand[b]	48.0	22.1	29.9	100	.69
Favor private ownership with high-quality goods, high prices, and inequality[c]	50.9	22.6	26.5	100	.61

Note: Eigenvalue equals 1.75 and explains 58% of the total variance. N = approximately 1,500.

[a] The question reads: "Let's talk about the possibility and necessity of radical economic reform in the Soviet Union which may lead to a free market economy. Some people say that such reform may at first lead to unemployment and inflation of prices after which may come plentiful consumer goods of high quality and economic prosperity. Do you think it is necessary to introduce such economic reform in the Soviet Union or not?"

[b] The question reads, "Are you in favor of an economy in which prices are set by supply and demand and people get jobs when they are qualified for them, or are you in favor of an economy in which prices are set by the government and people are given a job even though it may not be the one they prefer?"

[c] The question reads, "Are you in favor of a form of ownership in which there are many high-quality goods but not everyone has the same opportunity to buy them because of the prices, or a form of ownership in which prices of goods are low and all can obtain them, but goods are not available everywhere?"

prices and attempted, in early 1993, to reintroduce price controls. This is primarily because of the expected political fallout from the inevitable rise in prices of many goods that would follow liberalization (Gati 1990, 138–39; Sachs and Lipton 1990; Shiller, Boycko, and Korobov 1991).

Another critical element of free-market policy initiatives is a free market for labor. Under the command economy, employment was largely guaranteed by the state and individuals had constraints on the types of jobs they were allowed to take.[20] Free-market reforms will require the government to reduce its authority over employment decisions considerably and shift responsibility for finding and maintaining employment to the citizen. As a result, Soviet citizens are confronted with the real possibility of unemployment, which official figures suggested hardly existed prior to 1992 (Cook 1990; Sanjian 1990).

Finally, free-market reform includes the introduction of private ownership into an economy that has had limited historical experience with such a concept. Without extensive privatization many believe that these other initiatives would have little positive impact (Kornai 1990, chap. 4; Sachs and Lipton 1990). At the micro level this is expected to lead to greater efficiency of the Soviet firms, while at the macro level it should reduce government subsidies and bring down the budget deficit.

The dependent variable in this analysis combines all three of these important dimensions of the free-market policy debate in a single measure.[21] Each question was worded so as to present the respondent with a balanced view of the policy initiatives (see Table 2). The questions presented the respondents with both the positive and negative aspects of the policy initiatives. For example, price reforms would promote greater variety of goods and services but would result in dramatically higher prices and unemployment (at least in the short term). As many have pointed out, there is a tendency for Soviet citizens to want to have their cake and eat it, too—be spared the cost of free-market reforms but enjoy the reforms (Denisovsky, Kozyreva, and Matskovsky 1993; Zaslavskaya 1990a; Pipes 1990, 80). Thus, it is important to present Soviet respondents with policy options that clearly indicate the costs and benefits of such reforms.

Similar percentages of Soviet citizens give pro-market-reform responses to each of these three questions—approximately 50%. In every case, about 20% of the sample is undecided as to whether they support or do not support free-market reforms. Finally, for each of the questions, about a quarter of the sample is opposed to changing the economic system.

The percentage of the sample that gives free-market responses to all three of the questions gives a better approximation of core Soviet support for free-market reform. Of the sample 27.5% responded positively to all three questions, suggesting that about a quarter of the Soviet population stands strongly in support of free-market reforms.[22] A very similar percentage of the population (26.5%) stands firmly on the opposite side of the issue, strongly opposing such changes in the economic system (i.e., giving all antimarket responses). In 1990, support for radical economic change was, to say the least, lukewarm: almost a majority of the population seemed uncertain about free-market reform, and only 25% were strongly committed to these policies. The large number of citizens who express ambivalent preferences regarding free-market reform is not surprising given the political context in 1990. Gorbachev had strongly endorsed liberal political reforms, but he had made very limited moves toward promoting a market economy. Enterprises essentially remained in government hands; competition did not exist; and prices, although raised at the procurement and wholesale level, remained frozen and under government con-

TABLE 3

Retrospective Evaluations of Personal Financial Situation, European USSR, 1990

ITEMS	VERY IMPORTANT	IMPORTANT	NOT VERY IMPORTANT	NOT AT ALL IMPORTANT	DON'T KNOW	TOTAL
Shortages[a]						
Vital necessities	60.0	30.9	7.2	1.5	.4	100
Food goods	62.0	27.9	8.6	1.1	.4	100
Nonperishable goods	50.4	37.2	9.9	2.2	.3	100
Unemployment	33.7	30.2	20.1	13.1	2.9	100

Note: N = approximately 1,500.

[a] The question wording for all of these questions is, "Please look at the list on the showcard and answer how important each of the problems listed below are for you personally or for members of your family."

trol at the retail level (Dowlah 1992; Peck and Richardson 1991). With official policy remaining essentially antagonistic to the market economy, popular ambivalence to market reform is not surprising.

The dependent variable in the subsequent analysis is the factor score for each respondent that results from a factor analysis of these three questions. One significant factor emerges from a common factor analysis of the three items using principal axis factor extraction. The factor loadings of the three items are presented in Table 2.[23]

VARIABLES EXPLAINING SUPPORT FOR FREE-MARKET REFORMS

Subjective Assessments of Economic Performance

It was hypothesized that perceptions of a deteriorating economy would affect support for free-market reforms. The data leave little doubt that in May of 1990 Soviet citizens were unhappy with the economy.[24] Two measures of retrospective pocketbook assessments are included in the analysis and their marginals are presented in Table 3. One indicator consists of three questions concerning shortages of basic goods. Eighty percent of the respondents indicated that they considered all three shortages to be either important or very important for them personally or for their family. In the analysis that follows, a summary shortage variable consists of the respondent's mean score on these three shortage questions. Another indicator is unemployment. About two-thirds of the sample indicated that they considered unemployment to be either important or very important to them or their family, while a third of the sample indicted that it was either not very important or not at all important.

Soviet citizens clearly expected their personal financial situation to deteriorate in the near future. The following list indicates that the Soviet population had very serious concerns about their personal financial future.[25] An overwhelming 84% expressed a concern over the prices that they would have to pay for goods in the next twelve months. About half the sample expected that their family would live worse in the next year.

Item and Response	*Percentage*
Concern with rising prices in next 12 months	
Very worried	83.7
Somewhat worried	11.5
Not very worried	2.9
Not worried at all	1.2
Can't say precisely	.6
Refused	.1
Total	100.0
Family's living standard next year	
Much worse	22.9
Somewhat worse	23.9
No change	22.7
Somewhat better	12.1
Much better	1.6
Can't say precisely	16.6
Refused	.2
Total	100.0

Sociotropic measures, which are assessments of the overall national economy, were presented in the cross tabulations of Table 1. About 70% rated the economy as bad and only 15% expected it to improve in the following year. Hence both sociotropic retrospective and prospective evaluations of the national economy were uniformly bad.

These variables indicate an overwhelming negative assessment of economic conditions in the Soviet Union, regardless whether the reference point is the national economy or individuals' personal economic situation and whether the orientation is to the past or to the future.

Free-Market Culture

A second set of hypotheses proposed earlier suggested that support for economic reform might be the outgrowth of a Soviet free-market culture. Three dimensions of this culture are included in the analysis. One dimension of a free-market culture is belief about how free-market mechanisms operate, measured by the link respondents make between incentives (which of course imply inequality) and economic performance.[26] Given that Soviet officials

TABLE 4

Inequality and Economic Efficiency, European USSR, 1990

ITEM	AGREE	UNCERTAIN	DISAGREE	TOTAL	FACTOR
In order to get people to work better, we need to increase the inequality of incomes	**67.3**	15.7	17.1	100	*.56*
A great difference in incomes is necessary for the development of our society	**26.9**	29.2	44.0	100	*.41*
We are more likely to have a healthy economy if the government allows more freedom for individuals to do as they wish	**50.1**	22.6	27.3	100	*.34*
There ought to be more equality of income even if it means that some people will not work as well as they do now	25.8	15.5	**58.6**	100	*−.22*

Note: The "proinequality" response to these items is shown in bold. N = approximately 1,500.

practiced, until recently, a policy of wage leveling that promoted income equality, I might expect little recognition of the importance of incentives.[27] But evidence suggests that even citizens in command economies tend to accept the link between incentives and economic performance (Shiller, Boycko, and Korobov 1991; T. Smith 1989).

Table 4 presents the four items from the survey that measure *beliefs* of Soviet citizens about the link between inequality and economic efficiency. With the possible exception of the third item in the table, each of the statements expresses a clear trade-off between economic efficiency and the political goals of equality.[28] Three of the items command majority support for economic efficiency. The second item in the table is worded quite strongly ("a great difference in incomes is necessary") and commands the support of only about a quarter of the respondents. Generally, these results confirm that there is a high degree of support among Soviet citizens for meritocratic reward structures (McAuley 1980; Shiller, Boycko, and Korobov 1991).[29] For the purposes of the following analysis the respondent's factor scores resulting from a factor analysis of these four items serve as a measure of support for meritocratic reward structures.[30]

A second dimension of free-market culture pertains to individual *preferences* regarding equality. It is defined here as the degree to which individuals are willing to accept the abandonment of egalitarian goals. Egalitarian norms are paramount in the Marxist–Leninist philosophy that represented the foundation for official political culture in the Soviet Union (Kornai 1990, 21). It has been argued that this egalitarianism culture dates back to sixteenth-century Russia (Kennan 1986). And while wage differentials have been accepted as early as the Stalin period, significant differences in wages have been officially discouraged (Lane 1990, 32). Moreover, there is evidence that many Soviet citizens would oppose exaggerated inequalities that might result from liberal economic policies (Kornai 1990; Pipes 1990; Zaslavskaya 1990b, 105, 130).[31] These contentions are supported in Table 5, which presents the responses and factor scores for the three items measuring the degree to which respondents are willing to tolerate increased levels of

TABLE 5

Preferences for Egalitarian Policies, European USSR, 1990

ITEM	DISAGREE	UNCERTAIN	AGREE	TOTAL	FACTOR
The difference in incomes among people in our country is too great	**7.1**	8.2	84.8	100	*.83*
The government should try to equalize income levels	**17.9**	13.8	68.3	100	*.61*
Inequality among people will continue to exist as long as it is to the advantage of those who are rich and in power	**8.8**	13.2	78.0	100	*.38*

Note: The response opposing egalitarian policy is shown in bold. N = approximately 1,500.

TABLE 6

General Support for Government Social Guarantees, USSR, 1990 (%)

VARIABLE	USSR	AUS	USA	I	UK	NL	D	CH	A	H
Gov't should guarantee jobs										
Strongly disagree	1	7	9	2	3	2	3	4	4	1
Disagree	3	29	25	7	20	8	7	20	9	3
Uncertain	3	25	21	10	18	16	13	26	8	5
Agree	32	30	30	37	35	51	42	32	44	46
Strongly agree	62	9	14	44	24	24	36	17	35	45
Total	100	100	100	100	100	100	100	100	100	100
Gov't should guarantee income										
Strongly disagree	1	9	18	8	4	8	11	10	12	2
Disagree	6	32	41	14	23	24	19	29	20	8
Uncertain	7	22	21	12	13	19	17	19	10	12
Agree	40	31	15	41	40	39	36	30	39	39
Strongly agree	46	7	6	25	20	10	17	12	19	40
Total	100	100	100	100	100	100	100	100	100	100
Gov't should guarantee education to poor children										
Strongly disagree	0	1	2	1	1	1	2	0	2	2
Disagree	2	9	9	3	5	5	4	6	7	10
Uncertain	3	16	13	6	11	9	7	13	10	16
Agree	28	56	53	43	52	58	54	49	48	45
Strongly agree	68	18	24	46	32	27	34	32	33	28
Total	100	100	100	100	100	100	100	100	100	100
Approximate N	1,500	1,342	1,290	884	999	1,330	1,161	809	872	2,156

Source: The non-USSR data are from the 1987 International Social Survey Project (Dekker and Ester 1991).
Note: AUS = Australia; I = Italy; D = West Germany; CH = Switzerland; A = Austria; H = Hungary; NL = Netherlands.

inequality.[32] In the case of all three items, the responses are highly skewed in the proequality direction, suggesting that there is only limited support in the Soviet population for the inequalities associated with a free-market system.[33] Once again, the factor scores from a factor analysis of these three items are employed in the subsequent analyses as the measure for support for increased inequality.[34]

Another dimension of what I have called the free-market culture is attitudes toward government social guarantees. Transition to a free-market economy presumes that the extensive network of government guarantees that exist in communist societies would have to be curtailed (International Monetary Fund et al. 1991; Kornai 1990; Sachs and Lipton 1990).[35] I expect this attitude toward government guarantees to be an important delimiting feature of free-market culture because it is such a contentious point in the debate over economic change in these societies (Shmelyov 1991). The commitment of Soviet citizens to these social guarantees is illustrated in the conclusion Inkeles and Bauer draw from their study of Soviet émigrés in the early 1950s: "There appears to be a deep-rooted expectation among Soviet citizens that their government and society will provide extensive social welfare benefits, including job security, universal education, medical care, and other securities and guarantees" (1959, 236). Hence, respondents were asked to evaluate the importance of government job guarantees, income guarantees, and guarantees to support the education of children of poor families.[36]

The responses to these questions presented in the first column of Table 6 are highly skewed in the pro-social-guarantee direction. Soviet citizens overwhelmingly support the notion of government-guaranteed jobs (94% agreeing), government-guaranteed income (86% agreeing), and government guarantees of education for poor children (96% agreeing).

The comparative figures in Table 6 confirm the soviet citizen's strong support for government social guarantees.[37] Nevertheless, on two of the three indicators (job guarantees and educational guarantees for children) support of Soviet citizens for social guarantees is higher than levels found in Western nations, although not dramatically out of line. The Soviet Union has much higher support for income guarantees than all of the countries included in the sample. The highest percentage agreeing with this statement is found in Hungary (79%), the lowest, in the U.S. (21%). There remains overwhelming support in the Soviet Union for an activist governmental role, ranging from job guarantees and educational funding for children (which tend to receive strong support in many European countries) to guaranteed incomes, which receive much lower levels of support in Western nations.[38] In the analysis that follows, a composite index of these three variables is employed. The

TABLE 7

Support for Competitive Elections, European USSR, 1990

ITEM	DISAGREE	UNCERTAIN	AGREE	TOTAL	FACTOR
Those supporting competitive elections are doing harm to the country	**65.7**	26.6	7.7	100	*.53*
Competition between the communist party and other parties will improve the way the authorities work in the Soviet Union	13.8	21.1	**65.1**	100	*−.64*
A one-party system in the USSR promotes the development of democracy	**61.5**	22.6	15.9	100	*.70*

Note: The "prodemocratic" response to these items is shown in bold. N = approximately 1,500.

index represents the factor scores from a factor analysis of the three variables. The eigenvalue from the common factor analysis is 1.66, accounting for 55% of the item variance. The factor loadings are .7 for job guarantees, .55 for income guarantees, and .49 for educational guarantees.

Three dimensions of Soviet free-market culture are measured here. An overwhelming majority of the Soviet mass public recognizes the importance of the link between incentives and economic performance. A considerably smaller percentage of the public is willing to accept the inequalities that are inevitable by-products of free markets. Finally, there is virtually no support for a reduction in the social guarantees provided by the Soviet government.

Support for Democratic Reform

Those who have embraced democratic institutional changes are hypothesized to support free-market reform. The focus here is on support for specific democratic reforms and their relation to explicit free-market policies. Hence support for democratic reform is based on responses to a series of propositions regarding free elections in the Soviet Union.[39] In early 1990, this aspect of democratic reform was a very salient public issue because the nation had participated in its first relatively democratic election in 1989 and local and republic elections were held in 1990. The three propositions comprising this measure (presented in Table 7) indicate strong support for this aspect of democratic reform.[40] For each of the three propositions concerning competitive elections, about two-thirds of the sample registered their support. Support for democratic reform is the factor score that results from a factor analysis of each of these three items.[41] I should add, however, that on a number of other dimensions, Soviet citizens have also expressed high levels of support for democratic reform: overwhelming majorities indicated that political rights (such as freedom of speech) should always be respected, and most propositions measuring Soviet support for the value of dissent commanded at least majority support (Gibson and Duch 1993).

Care should be exercised in the conclusions drawn from these indicators. While the Soviet citizens express high levels of support for abstract institutions of democracy (such as free elections), on certain more concrete dimensions (e.g., the tolerance of political views they dislike or of public disorder), they can be quite antidemocratic (Gibson and Duch 1993).[42] This is also the finding of earlier research from the Soviet Interview Project (SIP) data (Grey, Jennisch, and Tyler 1990). Nonetheless, since the focus of this analysis is on preferences for institutional reform, the measure of support for democracy adopted here is institutional in nature.

MULTIVARIATE RESULTS

Table 8, column 1, presents the regression results for the pure economic model in which only the prospective and sociotropic economic variables are included in the equation.[43] Overall these economic variables explain 10% of the variation in support for free-market reforms. Both sociotropic and pocketbook evaluations of the economy are related to support for market reforms, but in very different ways. Those who think the present condition of the economy has deteriorated and those who expect it to be worse in the future are more inclined to *support* free-market reforms. Prospective pocketbook evaluations of the economy have exactly the opposite effect, contradicting my initial hypothesis. Those who expect their personal financial situations to deteriorate are more likely to *oppose* free-market initiatives. In particular, concern with future rising prices undermines support for these initiatives. Some care, however, is called for in interpreting this particular result because of the highly skewed nature of the price concern variable (84% answer in the affirmative). Retrospective evaluations of respondents' personal financial situation did not affect their attitudes toward free-market reforms.[44] With one exception, these results confirm my hypotheses regarding the economy and support for economic change. Negative evaluations of the overall economy, whether retrospective or prospective, seem to generate enthusiasm for free-market reforms. The exception is that those concerned about their future personal financial situation were less likely to support market reforms.

TABLE 8

Multivariate Model of Support for Market Reforms, European USSR, 1990

INDEPENDENT VARIABLES	MARKET REFORM MODELS 1	2	3	4	DEMOCRATIC REFORM MODEL 5
Sociotropic					
Retrospective					
Condition of economy[a]	.43**	.42**	.33**	.16**	.45**
	(.05)	(.05)	(.05)	(.05)	(.05)
Prospective					
Condition of economy[a]	.08*	.05	.04	.01	.07*
	(.03)	(.03)	(.03)	(.03)	(.03)
Pocketbook					
Retrospective					
Family living condition[a]	−.02	−.02	−.01	−.01	.01
	(.02)	(.02)	(.02)	(.02)	(.02)
Shortages[b]	−.03	−.01	.00	−.01	.01
	(.04)	(.04)	(.04)	(.03)	(.04)
Unemployment[b]	.00	.02	.01	.01	−.01
	(.02)	(.02)	(.02)	(.02)	(.02)
Prospective					
Prices will rise[c]	−.27**	−.21**	−.20**	−.17**	−.07
	(.04)	(.04)	(.04)	(.04)	(.04)
Live better or worse[d]	−.04	−.02	−.01	−.03	.05*
	(.03)	(.02)	(.02)	(.02)	(.02)
Free-Market Culture					
Oppose social guarantees	—	.07**	.06*	.05*	.03
		(.03)	(.03)	(.03)	(.03)
Oppose income equality	—	.07**	.06*	.05*	−.02
		(.03)	(.03)	(.02)	(.03)
Link inequality & efficiency	—	.27**	.28**	.22**	.17**
		(.03)	(.03)	(.03)	(.03)
Socioeconomic					
Ownership	—	—	.01	−.00	.02
			(.01)	(.01)	(.01)
Education	—	—	.10**	.05*	.15**
			(.02)	(.02)	(.02)
Age	—	—	−.01**	−.01**	−.01**
			(.00)	(.00)	(.00)
Industry	—	—	.01	−.03	—
			(.05)	(.05)	
Farming	—	—	−.06	−.03	—
			(.08)	(.07)	
Service	—	—	−.02	−.04	—
			(.05)	(.05)	
Democracy	—	—	—	.36**	—
				(.03)	
Constant	−.05	−.34	−.07	.47*	−1.47**
	(.20)	(.20)	(.21)	(.20)	(.20)
Number of cases	1,410	1,409	1,378	1,378	1,420
Adjusted R^2	.10	.17	.20	.31	.22

Note: Entries are unstandardized regression coefficients with standard errors in parentheses.
[a] High = bad.
[b] High = important.
[c] High = worried.
[d] High = worse
*$p < .05$.
**$p < .01$.

The variance explained increases to 17% in column 2, when the free-market-culture variables are added to the model. All three dimensions of free-market values are statistically significant and have the expected signs. Of the three dimensions, the inequality variable (which measures the perceived link between inequality and efficiency) has the biggest effect. Respondents who believe that inequality is a necessary condition for economic efficiency are more likely to support free-market reforms. Second, respondents who are opponents of government social guarantees are supporters of free-market reforms. Finally, those opposing policies that promote greater equality tend to be the proponents of market reforms.

The coefficients for the economic variables decline somewhat from those reported in column 1. Once again, none of the retrospective pocketbook variables are statistically significant. The sociotropic prospective variable is no longer significant. Only the retrospective sociotropic and the prospective pocketbook variables now reach statistical significance. This confirms that in addition to the importance of economic assessments, those who embrace free-market principles are more likely to support free-market reforms.

Table 8, column 3, presents the coefficient estimates once the socioeconomic self-interest variables are added to the equation. I restrict myself here to testing a very simplified version of the self-interested hypothesis developed earlier.[45] First, I have included age, education, and wealth (measured here by a count of the number of durable goods in the respondent's household). Second, I have included in the model three dummy variables that assume the value of 1 for individuals working in the service, farming and industrial sectors of the economy. (This leaves in the intercept term primarily individuals who receive government stipends, e.g., students and pensioners). Granted, these are rather rough categorizations of the working population. Nonetheless, they might give some indication of whether occupational groups were divided in terms of their support for free-market reform.

Adding these socioeconomic variables to the model increases the explained variance to 20%. The age and education variables are significant, but the ownership variable is not. None of the dummy variables for the occupational groups are significant in the model. I interpret the negative coefficient on age as support for the life-cycle argument that the young support free markets because they are in a better position to benefit from the market economy.[46] An alternative interpretation is that it reflects generational differences in values, as opposed to economic self-interest. This is how Bahry and Silver interpret the cohort differences in political attitudes and behavior (Bahry 1987, 73; Silver 1987). With the cross-sectional data available to date, however, it is impossible to distinguish between the life-cycle and generational interpretations. Moreover, I would argue that there is no strong theoretical rationale for expecting generational differences in support for free-market reforms. Until recently, official Soviet policies have been antagonistic to the free-market economy. Hence there has not been any significant formative event related to the market economy that could have impressed a particular generation of Soviet citizens.

The results for education reported in Table 8 also parallel the SIP findings. Silver (1987) discovers that the better educated are more likely to favor private as opposed to state ownership. Silver does not offer an explanation for why the better-educated are more likely to embrace this market reform. I believe that the correlation confirms that by 1990 when the debate over the costs and benefits of privatization had high visibility, better-educated citizens felt more comfortable about their ability to adjust to and benefit from this new economic order. The less-educated recognized that they would be more vulnerable in a new market economy because in a competitive labor market those with more education and training have a distinct advantage.

On the other hand, efforts to identify differential support among occupational groups failed. At least with this rather rough categorization of occupational groups, there is no evidence of differential support for free-market reform. Finally, there is no support for the hypothesis that the wealthy, who presumably have a bigger stake in the status quo, oppose reform. Those in a position of wealth in the old regime did not see themselves disadvantaged by the proposed economic reforms, possibly because the old elite was often very well placed in the case of privatization of state-owned entities.

Democracy and Free Markets

The Soviet results leave little doubt that preferences for democracy and free markets are highly intercorrelated. The Pearson correlation between support for competitive elections and for free market support is .47. Not surprisingly, when added to the regression equation, the explained variance jumps significantly to .31 (see Table 8, col. 4). The coefficients for the other independent variables drop somewhat from their values in the previous equation but those that were statistically significant remain so in column 4. Clearly, the partisans of democratic change are much more likely than opponents to support economic reform.

This strong correlation suggests that Soviet preferences for democratic reform are the result of similar considerations that shape support for economic reform. I test this proposition by regressing the same set of variables from the free-market model on the support-for-competitive-elections variable. As the results in column 5 illustrate, the two sets of preferences are shaped by similar factors. Concern with the economy today (the retrospective sociotropic variable) has a very strong correlation with democratic reform, as is the case in the free-market equation. Dissatisfaction with the recent performance of the overall economy promotes support not only for economic change but also for democratic reform. There is also a significant correlation between the sociotropic prospective variable and the democracy measure, which is not the case in the market equation. With respect to pocketbook concerns, the two sets of results diverge somewhat. In the democratic equation, concern about future rising prices is not significant but the variable has a significant negative correlation in the market equation. Another difference is the moderately significant correlation between concern about future living standards and support for democratic reform: in the market equation, this variable has no effect. Generally, the impact of prospective pocketbook concerns on democratic preferences appears to be even less than in the case of free-market preferences.

As was the case in the free-market equation, there is a relationship between free-market values and support for democracy but the relationship is much weaker. Respondents' acceptance of the link between inequality and efficiency is significantly correlated with support for democratic change but the size of the coefficient is smaller. Moreover, neither of the other two free-market culture variables are significant in this equation. Hence there is a connection to democracy, but it is not nearly as powerful as that to market reform. Socioeconomic variables are similarly related to support for democratic reform: both age and education are significant and in the expected direction, while the wealth variable is not.

Clearly, preferences for changing both political and economic institutions are part of a similar evolution of public opinion in the Soviet Union. They are both supported by better-educated and younger citizens, more prevalent among those concerned about a deteriorating economy, and more likely found among individuals who recognize the link between inequality and efficiency. This is a surprising finding. I would have expected more differentiation between the two processes, with economic factors (assessments of economic performance and free-market culture) important in the case of free-market preferences but relatively insignificant in the case of support for democracy. A reasonable conclusion is that preferences for free-market and democratic institutions tend to have a strong reciprocal relationship and that both are shaped by similar factors.

These empirical findings have important implications. The transition to a free-market economy seems to be built on a foundation of a free-market culture. Respondents who accept the link between inequality and economic efficiency are much more likely to endorse free-market reforms. More importantly, those who do not accept this link (and many in the Soviet Union do not) are likely to oppose free-market reform. It is interesting to note that this same variable is also correlated with support for democratic reform, although not as strongly as with market reform. Democrats tend to accept the inequalities associated with the workings of a market- and incentive-based economy.

In addition to creating hardships, the transition to a free-market economy will create opportunities for many citizens. Those most likely to benefit from these economic changes (the better-educated and the young) are among the strongest free-market advocates. Once again, although for different reasons, the results for the democratic reform model parallel those for the free-market equation.

This notion that democrats and free-marketers are both cut from similar cloth contradicts some of the recent findings of Finifter and Mickiewicz (1992). They find that the better-educated and those supporting political change (which can be interpreted as favoring democratic institutions) tend to "be more supportive of state responsibility," which the authors seem to equate with opposition to economic reform. First, it should be pointed out that attitudes toward the responsibility of the state for the well-being of individual citizens are not synonymous with support for a market economy. (In many European countries, citizens support the market economy but also favor a strong role for the state.) In this analysis, I treat general attitudes toward the role of the state as part of the free-market culture and distinguish them from preferences for the implementation of free-market institutions.

But this caveat aside, the puzzle remains because my free-market-culture variables also all have exactly the opposite correlation with education and democracy than was the case with Finifter and Mickiewicz (1992). Education and support for democracy have the following relative correlations with opposition to social guarantees (.10 and .07), with the recognized link between equality and efficiency (.05 and .16), and opposition to equality of income (.11 and .09). In all cases, the direction of the correlations indicate that the more educated and more democratic rank higher on the measures of free-market culture which is contrary to Finifter and Mickiewicz's findings. The differences may be a function of measurement. Finifter and Mickiewicz base their conclusion on a single-item indicator of support for an activist state. The market-economy variable and the three free-market-culture variables employed in this analysis are all measured with multiple indicators. It seems unlikely that by chance these three different multiple-indicator constructs would all have the expected correlations with education and support for democracy. It is true though that the "social guarantees" dimension of free-market culture, which is most similar to Finifter and Mickiewicz' "locus of responsibility" variable, is not significantly correlated with support for democratic reform in Table 8. Hence, it may be that the particular dimension of free-market culture favored by Finifter and Mickiewicz is uncorrelated (or possibly negatively correlated) with support for democracy.

On the other hand, my results are consistent with the positive correlation that Finifter and Mickiewicz report between their "inequality and efficiency" variable and other variables measuring support for political change (eg., competitive elections).[47] Democrats accept the link between an inefficient market economy and social inequality. This is particularly noteworthy because the inequality-and-efficiency variable is the one free-market-culture dimension that is significant in my democracy equation in Table 8.

In large part, Finifter and Mickiewicz' (1992) findings can be incorporated into my general finding that citizens in the former Soviet Union support a decidedly social democratic version of capitalist democracy—one that recognizes the importance of supply-and-demand prices, private ownership, and income inequality but at the same time values a strong social and economic safety net. Because citizens of the former Soviet Union strongly cling to the notion of a social and economic safety net, the social guarantee variable in my model is unrelated to support for democracy and has a negative relationship in the case of Finifter and Mickiewicz.

CONCLUSION

In the early part of 1990, public opinion in the European USSR seemed unenthusiastic about the free market. The results suggest that such reforms had solid support among a quarter of the population and were strongly opposed by a similar proportion, leaving half the sample with mixed feelings. But I am here less concerned with levels of support for the free market; rather, the central focus is understanding how these preferences are shaped.

An extensive body of literature argues that political preferences are formed by instrumental calculations: citizens assess policies, incumbents, or even regimes in terms of how they impinge on their economic well-being. This hypothesis is supported here by the correlations between assessments of the economy and free-market preferences. Moreover, the nature of the relationship is exactly as the literature predicts. In 1990, those most critical of past economic performance tended to be the most enthusiastic supporters of free-market reform. Moreover, what seemed to matter most in these retrospective evaluations was assessments of the performance of the overall economy rather than the respondent's personal financial predicament. The negative relationship between retrospective assessments and free-market support suggests that the market economy was seen as a palliative for the economic decline that characterized the preceding two decades. Nevertheless, there is some limited evidence that concern with a future deterioration in personal finances might contribute to rejection of free-market experiments.

In a similar manner, citizen support for *democratic* reform is enhanced by memories of poor overall economic performance in the past and by expectations that the overall economy will decline in the near future. Further democratization seems to be considered an acceptable antidote to economic mismanagement. To some extent, this reflects the fact that in 1990, Soviet citizens continued to see the influence of the previous regime on economic policymaking. After all, an unelected president continued to dictate policy and the Soviet parliament was dominated by a party apparatchiks who persistently opposed major economic change. Hence, there is some suggestion that institutional reform was seen as a means to reduce the influences of the former regime over economic policymaking. Pocketbook issues played only a weak a role in shaping support for democratic reform. But unlike the market reform case, deteriorating prospective assessments of personal living conditions tended to bolster support for political reform. Therefore, I conclude that democratic reform in the former Soviet Union benefited from perceptions of deteriorating economic conditions both at the societal and personal level.

An interesting issue that can only be addressed in future research concerns how long individuals are likely to blame the declining economic situation on the policies of the previous regime. The evidence presented here concerns a very early period in the transition to democracy and free markets. If memories of the poor economic performance of the communist regime are quickly discounted, the positive relationship between perceptions of economic decline and support for market reform may weaken, and possibly reverse signs, in the very near future. Research from the Eastern European countries suggests that the discount rate may be quite high (Bruszt and Simon 1992).

Economic reform appeals to the young and well-educated, providing some limited confirmation for the notion that support for economic reform is instrumental: citizens who have the resources to benefit from such policy changes are likely to be its strongest advocates. But an effort to detect any differential support among sectors of the Soviet work force failed. There are two explanations for these findings that cannot really be distinguished by means of the data at hand. One possibility is that the economic sector is poorly specified, rendering the model unable to capture the true self-interested effects. Another possibility is that respondents are unable to evaluate their own self-interest associated with free-market reform, which is plausible given the novelty of these initiatives for the Soviet citizens.

Beliefs about how market mechanisms work (part of the free-market culture) affect how far Soviet citizens will accept free-market reforms.[48] Those who recognize the link between inequality and economic efficiency were more likely to embrace the free market. Even though the inequality and economic efficiency questions received the most pro-market responses, only between one-quarter-to-two-thirds of the respondents endorsed this idea. The other two dimensions (support for social guarantees and for egalitarianism) are more weakly correlated with support for free-market reform *and* command the support of only a small percentage of the population. This suggests that the transition to free markets in the Soviet Union will have to accommodate a large proportion of the population that clings aggressively to the notions of social egalitarianism. To the extent that public opinion matters in this transition process, the free-market model for the former Soviet Union will more likely resemble that of the social democracies of Western Europe, which tend to champion egalitarianism, rather than the more laissez-faire model found, for example, in the United States.

The hypothesized link between democratic values and support for free-market reform is strongly endorsed by the results of this study. Moreover, the two concepts are shaped by very similar factors. Although they are not synonymous, many citizens seem to consider the two concepts to be very similar. In many cases, what drives the individual to embrace democracy also results in a sympathetic reception for the market economy. While the support for both types of institutional reform are strongly correlated, levels of support for democratic reform remain significantly higher than they are for free-market reforms. There is a large element of the Soviet population that embraces democracy but continues to be quite reticent about the market economy.

There is a tendency, particularly among political leaders in the former communist nations, to argue that too much democracy will seriously inhibit the transition to a free-market economy. It is true that the existence of democratic institutions forces leaders to take stock of public attitudes toward these reforms. But measures designed to curtail the democratic process in order to facilitate the transition to free markets may actually undermine popular support for these reforms. Support for democratic norms is clearly an important correlate of popular acceptance of the free market. Just as individuals are inclined to embrace the free market as part of their support for democracy, any decision to put "democracy on hold" might, by the same logic, undermine public support for economic change. The transition to a free-market economy would be better served by efforts to accelerate rather than slow down the process of democratization. Historically, democratic institutions were seen as an important means to guarantee the individual liberties associated with a well-established free market. In the case of the Soviet Union and possibly other former communist regimes, widespread popular support for democratic values may be the vehicle for galvanizing popular acceptance of the free market.

The process by which institutions and attitudes change is very different in the late twentieth century than it was during earlier historical periods. Certainly in the early history of most of the developed democracies, democratization was typically preceded by an economic change that either gradually or abruptly introduced private property and free-market institutions. The situation is made much different today because of the promotion of different models that are easily accessible through various global mass media (Dahl 1971; Pye 1990). Unlike earlier periods, democracy has emerged as a popular institution not because it best facilitates the protection of liberties, private property, and contracts, which are such valued aspects of a free-market economy. Rather, the democratic model is popular in the Soviet Union because of its inherent attractions (freedom, liberty, etc.) and widespread diffusion through the global media—which Starr labels the "global demonstration effect" (1991, 360; see also Bruszt and Stark 1991; Mueller 1989). To some extent, democracy is something of a fad in the Soviet Union, promoted through increasing access to Western ideas.

Capitalism, on the other hand, has not benefited from the same positive public relations and this is reflected in the lower levels of enthusiasm that Soviet citizens have demonstrated toward free-market values. Market economies come in wide varieties and have been subject to widespread criticism both in the West and in the former communist regimes (in contrast to the value of democracy, which is rarely challenged). Hence, because the global portrayal of the free-market model is much more contentious than is the case with democracy, it should come as no surprise that the Soviet population demonstrates a certain hesitancy regarding free-market reforms. Moreover, the changes associated with free-market reforms will be exacting and pose serious short-term risks for the average citizen.

The findings presented here suggest that the citizens who have embraced democratic values are also likely to support the adoption of free-market reforms. Since there seems to be a high degree of support for democratic institutional reform, this bodes well for proponents of free-market policies. To some extent, the contemporary developmental relationship between free markets and democracy has changed from what existed in the previous periods. While at the system level, free markets have historically been considered a necessary condition to democratization, the pattern has reversed itself somewhat today. At the individual level, the two variables—acceptance of certain democratic norms and support for free markets—are mutually reinforcing. Therefore, proponents of free-market reform can take some comfort from the widespread acceptance of democratic institutions. But the results also suggest reason for concern. If support for democratic institutions were to wane (as it very well might in a number of the newly independent republics), it could have adverse consequences for the introduction of market economies.

Notes

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1. I realize that recent events have rendered the term Soviet Union somewhat outmoded. For convenience sake, I retain the term as a general reference to the mass publics and republic-level political institutions that occupy the former territories of the Soviet Union.

2. The joint report of the International Monetary Fund and other international financial institutions recommended radical economic reforms and noted that "the prospect of a sharp fall in output and rapid increase in prices in the early stages of a radical reform is daunting" (1991, 18). A discussion of some of the social consequences of restructuring can be

found in Zaslavskaya 1990a, 86–95. See also the collection of essays in Bahry and Moses 1990. For a discussion of these issues from the perspective of Third World development, see Nelson 1990a.

3. Offe discusses why he thinks the micro preconditions for the development of free-market institutions (specifically, patience on the part of the mass public) do *not* exist in Eastern Europe and the Soviet Union (1991, 22). For a discussion of the politics of economic change in developing nations, see Nelson 1990a, 1990b.

4. For a discussion of why such free-market initiatives have gained so much popularity, see World Bank 1991.

5. This point is made by J. Elster (1990) and reinforced by Offe (1991).

6. The link between the economy and political preferences is often made in the U.S. case. Two of the most thorough summaries of this voluminous literature are Kiewiet 1983 and, for the comparative perspective, Lewis-Beck 1988.

7. Others, such as Sanjian, have argued though that the fear of change may override any support for free-market reforms: "The citizenry as a whole, independent of any individual's place in the power structure, fears the prospect of higher prices as subsidies are eliminated and supply and demand move prices to their market levels, resents the growth of social tension as income distribution becomes less egalitarian, and protests the weakening of familiar safety nets for the disadvantaged. Though they will also enjoy the fruits of a healthier economy in the long run should the reforms prove effective, it is these short-term costs—together with the absence of any apparent immediate benefits of perestroika—that preoccupy public opinion" (1990, 62).

8. One of the problems with modeling prospective evaluations of the economy with preferences (support for an incumbent or particular policies) is the difficulty in characterizing the exact reasoning of the respondent. Are respondents projecting the future impact of old policies on the performance of the Soviet economy or have they already factored into consideration a change in economic policy so that they are responding to the future performance of free market policies? With appropriate questions, greater light could have been shed on these issues but unfortunately they were not asked in this particular survey project. Hence my model of citizen preferences assumes that in May 1990, when asked to assess the general state of the Soviet economy in the next 12 months, citizens assigned responsibility to the command economy policies of the former regime.

9. *Perestroika* can simply be translated as "restructuring." But during the Gorbachev period the term became associated with a wide range of political and economic reforms such as attempts to decentralize enterprise decision making and increase the financial independence of state-owned firms; and the introduction of laws authorizing cooperative enterprises in the services-and-consumer-goods areas and permitting joint ventures with Western firms.

10. Also see Brody and Sniderman 1977 and Schlozman and Verba 1979 for evidence of the general tendency of individuals to personalize problems, rather than attribute them to societal forces such as government.

11. Presumably, in a market economy with private firms, employee compensation would be based on a competitive labor market. If employees were allowed to continue unconstrained to appropriate goods and services from the firm, as is the case now in many state-owned firms, the end result would be bankruptcy. This behavior is, of course, a form of rent seeking that occurs in market economies (see, e.g., Krueger 1990). But rent seeking has been particularly prevalent in certain sectors of the Soviet economy. With privatization and freeing of prices, opportunities for such lucrative activity would be reduced and therefore free-market policies would likely be opposed by employees in these sectors.

12. For a discussion of the role of socioeconomic interest in Hungarian public opinion during its regime change, see Bruszt and Simon 1990.

13. An illustration is Kornai's discussion of the cultural barriers to economic reform in Hungary:

> At each stage of his life, starting with the child entering kindergarten and ending with the old person retiring to a home for the aged, the citizen of a socialist country was told that not business, but only work (more specifically, work done in the framework of an enterprise or organization in the public sector) was the single legitimate source of income. He was taught that some inequality was tolerable or perhaps even useful for the sake of providing material incentives to people, but that there should not be "too much" of it. . . . Right now, in the beginning of a new era, many people in various political groups, even within strongly anticommunist movements, are still under the spell of their former indoctrination in extreme egalitarian values. They regard profit or high income as the result of unethical practices, and speculation and profiteering as sure signs of unacceptable greed. (1990, 21)

14. For an excellent short summary, see the essay by Almond (1991). See also Berger 1986; Dahl 1989; Lindblom 1977; Moore 1966; and Schumpeter 1942.

15. The argument that free markets and the abandonment of socialism are important preconditions to the development of democracy has been made by a number of Soviet scholars (e.g., Klyamkin 1990; Migranian and Klyamkin 1990).

16. This is the classic liberal case for market systems: "Liberty through the market; no liberty without." See Friedman 1962; Hayek 1944; and Lindblom 1977, 45. See also the extensive evidence of the strong correlation between economic development (which tends to be synonymous with market economies) and democratization (Dahl 1971; Lipset 1959; Powell 1982).

17. Polina Kozyreva and Gennady Denisovsky directed the fieldwork. A more extensive description of the survey is provided in Gibson and Duch 1993.

18. For a similar effort to characterize support for free-market policies in the Soviet Union, see Times Mirror Center for the People and the Press 1991, a preliminary report of a survey conducted in May 1991.

19. See also a discussion of the Shatalin Plan and the linkage between free prices and a decentralized economy in Hewett 1990 and Peck and Richardson 1991.

20. Cook (1990) notes that three important factors explain high levels of job security in the Soviet Union: management incentives to retain and hoard labor, institutional and legislative guarantees of employment security, and an implied social contract between the regime and its citizen. A discussion of the right to work in socialist and capitalist economies can be found in Elster (1988).

21. In this respect, it differs considerably from other studies such as the recent one conducted by Schiller, Boycko, and Korobov (1991), which is not concerned with individual preferences regarding free-market policy initiatives but, rather, with how individuals would respond to a variety of different free-market situations.

22. This 28% is precisely the percentage that Pipes cites as supporting a free market "in the true meaning of the words" (1990, 80).

23. The mean inter-item correlation for the three questions included in this scale is .35. Cronbach's alpha for overall scale reliability is .58.

24. With events changing so rapidly in the former Soviet Union, it would be imprudent to treat a single survey as more than a snapshot of an evolving political situation. This evolving situation is less of a problem here because our primary interest is the relationship among variables.

25. N = approximately 1,500. The question wordings were, "How worried are you that the prices of the things you buy will rise sharply in the next twelve months?" and "And what about next year? Do you think you and your family will live better than today or worse?"

26. For an excellent comparison of the popular attitudes toward and knowledge of free-market principles in the Soviet Union and the United States, see Shiller, Boycko, and Korobov 1991, 1992.

27. Evans (1990) notes that this policy of aggressive wage leveling actually originated in the 1950s under Khrushchev. But by the 1980s the need to link rewards with the quality and quantity of labor output was even embraced by Communist

party officials (Evans 1990; Shmelyov 1991; Zaslavskaya 1990a).

28. In each case, respondents were asked to indicate how far they agreed with the proposition. The response set was *agree strongly, agree, uncertain, disagree,* or *disagree strongly.*

29. Shiller, Boycko, and Korobov (1992) suggest that Soviet attitudes toward market incentives are not easily distinguished from those of citizens in developed capitalist nations. Given similar situational or institutional situations, the two citizen types are likely to behave similarly. They suggest that what varies between the two types of societies is the institutional and regulatory environment, leading to different types of economic behavior. This is consistent with results reported here that indicate a basic understanding and acceptance of how market mechanisms operate.

30. The eigenvalue of the first factor extracted is 1.44, accounting for 36% of the original variance. The eigenvalue of the second factor extracted is .94. N = 1,560. I noted earlier that measures of free-market values are quite distinct from preferences for free-market reform. Aside from the theoretical justification for treating these as distinct concepts, the empirical evidence also suggests that they are very different. The correlation between the free-market factor score and the "inequality and efficiency" factor score is .26.

31. The conflict between egalitarian norms and efficiency in capitalist democracies has been widely debated. See Okun 1975; Schumpeter 1942; and Zajac 1978.

32. The response set read as follows: *disagree strongly, disagree, uncertain, agree, strongly agree.*

33. These results contrast rather dramatically with the support for income equality found in the Finifter and Mickiewicz study (1992, 860). They find strong opposition to egalitarianism while I find exactly the opposite. I tend to concur with the authors that their question wording probably biased respondents *against* income equality.

34. The eigenvalue of the first factor extracted is 1.71, accounting for 56.9% of the original variance. The eigenvalue of the second factor extracted is .80. N = 1,560.

35. For a discussion of these extensive social guarantees, see Zaslavskaya 1990a.

36. These indicators have been adopted from the International Social Survey Project (ISSP) on attitudes toward social inequality (see Dekker and Ester 1991; Jowell, Witherspoon, and Brook 1989).

37. The wording in the ISSP study is slightly different from that used in the USSR questionnaire. The ISSP propositions were "The government should provide more chances for children from poor families to go to university," "The government should provide a job for everyone who wants one," and "The government should provide everyone with a guaranteed basic income."

38. In their analysis of analysis of Hungarian mass attitudes toward government social guarantees and welfare policies, Bruszt and Simon (1991) find similar high levels of support for government interventionism.

39. From the battery of propositions posed in this survey regarding democracy, this particular set focused most clearly on specific institutional reform.

40. This measure of support for competitive elections was developed with James Gibson and Kent Tedin (see Gibson, Duch, and Tedin 1992).

41. The eigenvalue of the first factor is 1.77 and accounts for 59% of the item variance. The eigenvalue of the second factor is .68. Cronbach's alpha is .66.

42. In fact, in two earlier works, employed a measure of democratic values that incorporated a wide range of dimensions in a single indicator (Gibson and Duch 1992; Gibson, Duch, and Tedin 1992). Here I separate out support for specific institutional reforms from other dimensions of democratic values. My primary theoretical concern here is to explore the relationship between support for two different types of institutional reform: changes in the economy and change in political institutions. Support for competitive elections proved a particularly good indicator of Soviet support for the introduction of democratic institutions. The measure of support for competitive elections employed here is highly correlated with the democratic values I employed elsewhere (Gibson and Duch 1992). The Pearson correlation coefficient is .79. Moreover, in the subsequent analyses the results are essentially the same regardless of which of the two measures is employed.

43. The dependent variable is based on the factor scores from the factor analysis described earlier. High values represent support for free-market reforms.

44. Simply on the assumption that respondents' evaluations of the future might be highly conditioned by their assessments of the past or that assessments of the overall economy and their personal financial situation might be strongly correlated, one might expect a high degree of multicollinearity among the economic variables in the model. But, this does not prove to be the case. The highest correlation among the economic independent variables is −.43 between assessments of next year's overall economy and whether the respondent thought he or she would live better next year. Dropping this variable from the analysis has very little impact on coefficients and their standard errors.

45. Zaslavskaya (1990a) suggests a very detailed set of hypothesis linking the present socioeconomic situation to self-interested attitudes toward market reform. Unfortunately the measures of socioeconomic status in the USSR survey are not precise enough to test these hypotheses.

46. Finifter and Mickiewicz (1992) report a similar finding and include "career advantages" as at least one explanation for this relationship.

47. In Finifter and Mickiewicz 1992, app. B, this question is listed as Q22b.

48. The notion that norms play an important role in economic behavior is addressed by Elster (1989).

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THE ESCALATION OF GREAT POWER MILITARIZED DISPUTES: TESTING RATIONAL DETERRENCE THEORY AND STRUCTURAL REALISM

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Realism has been the dominant paradigm in the study of international conflict. Within this paradigm, two leading alternative approaches have been deterrence theory and structural realism. We test the relative explanatory power of these two theoretical approaches on the escalation of deterrence encounters among great powers from 1816 to 1984. We derive a set of hypotheses from each model, operationalize them for systematic empirical analysis, and test the hypotheses on 97 cases of great-power deterrence encounters by means of probit analysis. The results are that the hypotheses derived from deterrence theory receive considerable support, whereas none of the hypotheses derived from structural realism are supported.

In contemporary research on the causes of international conflict, the realist paradigm has been the dominant theoretical approach. Within this broad framework, however, scholars have developed at least two prominent alternative models of the conditions under which conflict is likely to arise. The first approach, structural realism,[1] focuses on the attributes of the international system while the second, deterrence theory, is largely dyadic and emphasizes the resolve and relative military capabilities of adversarial states. While both theoretical approaches attempt to identify the conditions under which international crises and wars are likely to arise, their critical difference lies in the variables they argue to be the most important in determining the decisions of state leaders. More specifically, the two models emphasize variables from different levels of analysis. We shall test the relative explanatory power of variables from these two models in predicting the escalation of militarized disputes among great powers.

Many scholars have empirically tested systemic theories of conflict behavior. (For a listing of relevant works, see Huth, Bennett, and Gelpi 1992, 479.) Taken as a whole, however, these studies have not identified a robust empirical relationship between system structure and international conflict. One possible reason for the lack of clear findings is that these studies have not properly specified the theoretical connection between system structure and the decisions of policymakers at the state level. In a previous paper, we specified that connection and found that system structure had a significant impact on great-power militarized dispute initiation (Huth, Bennett, and Gelpi 1992).

In contrast to most quantitative research on system structure, a number of empirical studies have lent support to the explanatory utility of deterrence theory (e.g., Bueno de Mesquita 1981a; Huth 1988; Mearsheimer 1983; Wu 1990). One possible reason is that deterrence theory has not suffered as acutely from the problem of underspecification, since it is focused at the decision-making level of analysis. However, one important shortcoming of these empirical studies is that they have not tested deterrence theory against structural realism, its prominent competitor.

Drawing on our previous work (Huth, Bennett, and Gelpi 1992), we shall establish a clear logical connection between system structure and the decisions of state leaders. We shall test this model against a simple deterrence model to see which one provides greater explanatory power. Our more complete model of system structure builds upon the work of Bueno de Mesquita (1978, 1981), who has argued that system structure relates to conflict escalation through its interaction with the risk propensities of decision makers. In contrast to our previous findings regarding dispute initiation, in this study we find—in opposition to much of conventional wisdom but consistent with the argument we develop—that even when properly specified the structural realist model has *no significant* explanatory power with regard to the escalation of conflicts between great powers. Deterrence theory, on the other hand, provides *substantial* insight.

First, we define the concepts that are central to our theoretical models and formulate hypotheses derived from both structural realism and rational deterrence theory. Then we discuss the research design for testing the model and the operationalization of the variables and present the results of our empirical tests. Finally, we consider the theoretical and policy implications of our findings.

DEFINITION OF CONCEPTS

Structure of the International System

A self-contained system can be thought of as a set of interacting and interdependent units. Both the ordering of the units according to power resources and the density and arrangement of linkages among those units determine the structure of a system. This structure, in turn, may give rise to conditions that result in recurring patterns of unit behavior. The application of this general definition has created some disagreement among scholars concerning what specific features comprise the structure of the international system. We shall present the range of debate regarding this issue and test the empirical utility of various operational definitions of system structure.

We have identified six differing conceptions of system structure in the literature. The narrowest conception of system structure is that of Kenneth Waltz (1979), who asserts that we should distinguish between international systems according to the number of great powers. In fact, he narrows his definition further to argue that the only important distinction is between systems with two great powers (bipolar) and those with four or more great powers (multipolar).[2] William Thompson (1988), however, argues that the distribution of capabilities among great powers is important, in addition to their number. Furthermore, some scholars argue that alliance coalitions are also important in shaping structural incentives for conflict (e.g. Deutsch and Singer 1964). Additionally, if one accepts the distributional logic presented by Thompson as well as Deutsch and Singer's assertion that alliance groups represent important systemic actors, then the distribution of capabilities among alliance coalitions should also be a systemic variable. Finally, some scholars have argued that the degree of cross-cutting ties between alliance blocs should also be considered an aspect of system structure (Bueno de Mesquita 1975; Deutsch and Singer 1964).

Given our general definition of system structure, we see no logical reason to exclude any of these alternative components of system structure, and so we shall consider all of them. Furthermore, in the face of such disagreement it is important to test for the robustness of our results.

System Uncertainty

System uncertainty is conceptualized as the confidence that decision makers have in their estimates of the expected outcome of an armed conflict resulting from characteristics of system structure. The level of system uncertainty refers to the amount of variance around a decision maker's estimate of an outcome—win, lose, or draw.[3] It follows that when system uncertainty is low, decision makers will be relatively sure about what the outcome of an armed confrontation will be. Conversely, when system uncertainty is high, decision makers will find the outcome more difficult to predict. System uncertainty is necessarily a perceptual variable, but some of the uncertainty that decision makers experience is a direct function of the structural attributes of the international system.[4] We shall develop the relationship between system structure and system uncertainty in the next section.

Risk Propensity

Risk propensities reflect the fact that different individuals may choose differently because of their attitudes toward options with probabilistic outcomes.[5] For example, assume that there are two alternatives with the same expected value. A risk-acceptant actor will prefer the alternative with a high payoff but a low probability of receiving that payoff, whereas a risk-averse actor will prefer to receive a lower payoff with a higher level of certainty (Luce and Raiffa 1957).[6]

We believe that there are two important sources of risk attitudes: individual and situational. In the former, personality characteristics predispose individuals to take or avoid risks. In the latter, risk attitudes vary with the situational context in which decisions are made (see, Kahneman and Tversky 1979; Quattrone and Tversky 1988). Specifically, when individuals frame their choice of options from the perspective of trying to avoid losses, they are likely to be risk-acceptant. Conversely, when the options are viewed as an opportunity to make gains, individuals will be risk-averse.[7]

Deterrence

We define deterrence as a "policy that seeks to persuade an adversary, through the threat of military retaliation, that the costs of using military force will outweigh the benefits" (Huth 1988, 15). In a situation of immediate deterrence, a challenger is *actively* considering the use of military force, and the target counters with a threat of military retaliation. Deterrence may be undertaken either in defense of a state's own territory (direct immediate deterrence) or that of another country (extended immediate deterrence).

THEORETICAL MODELS AND HYPOTHESES

In our theoretical models of system structure and deterrence, we present in simplified form the key variables expected to shape the decisions of a challenging state that is considering whether to escalate a dispute and risk an armed conflict. Each model assumes that states behave as unitary rational actors. That is, each argues that the leader in the challenging state chooses between accepting the status quo and escalating a conflict based on the expected value of armed conflict versus the payoff expected from the status quo.[8] Once again, the important difference between the approaches lies in the variables they argue to be salient in leaders' cost–benefit calculations.

Conflicts of interest between states rarely lead directly to war. More often, interstate disputes escalate to the outbreak of war in two stages: the initiation of a militarized dispute and the escalation of that dispute into a war. Logically, both system structure and the deterrent policies of states should be relevant at each stage of this escalatory process. We define militarized dispute initiation as the movement from peaceful competition between adversaries to an attempt by one state to overturn the status quo by coupling demands with threats of force. If the challenge is resisted and an international crisis emerges, then the analysis shifts to militarized dispute escalation. At this stage, decision makers must choose whether to stand firm and resort to the use of force to secure their interests or back down in order to avoid the costs of war. In previous works, we have analyzed the first stage of this escalatory process (Huth, Bennett, and Gelpi 1992; Huth and Russett 1993). Here, we shall focus on the second stage.

The study of escalation can only follow the initiation of a militarized dispute. As a result, challenging states select themselves into our population of study, making it important to consider the effects of this selection process on our theoretical and empirical analyses. Generally, we expect that relatively resolved states will be selected into our population. Such resolved states will be more likely to escalate disputes regardless of the deterrent policies of its adversary or the effects of system uncertainty. The implications of this selection process for our empirical analysis is that in both models, positive coefficients should be biased downward, while negative coefficients should be biased upward (Achen 1986).[9] However, as long as one does not try to generalize beyond the particular stage of the selection process under analysis, we think it is misleading to speak of this effect as selection bias within our study. Our estimates *do* accurately reflect the influence of the different variables at *this stage* of the selection process.

Structural Realist Hypotheses

Wars are initiated by state leaders, so if system structure is to affect conflict behavior, it must do so through its effects on decision makers. Structure provides information to leaders about the expected outcome of an armed conflict. Some systemic conditions lead state leaders to be more confident in their predictions of conflict outcomes, whereas others force them to be less confident. As a result, at the core of the structural realist perspective is the link between decision makers' uncertainty created by the structure of the system and decision makers' willingness to engage in conflictual behavior. We assume that leaders do not have perfect theories explaining the conflict behavior of other states and that therefore, decisions regarding the escalation of a conflict will always be made in the face of uncertainty. When leaders must make decisions in an uncertain environment, their propensity to take risks will necessarily shape their behavior. In particular, the effects of system uncertainty will be mediated by decision makers' risk-taking propensities.[10] When system uncertainty is high, risk-acceptant decision makers will gamble by pursuing policies that run the risk of provoking armed conflict. Risk-averse decision makers, however, will be cautious regarding the escalation of disputes when faced with high levels of uncertainty. Thus uncertainty should have *opposite* effects on a state's escalatory behavior, depending upon the risk propensity of its leaders.

In order to move from this general argument to specific testable propositions about conflict behavior, we must apply this argument to each of the six conceptions of system structure identified earlier. First, we will address the conceptions of structure that focus on the number of actors in the international system, that is, Waltz's division between bipolar and multipolar systems, the number of great powers, and the number of alliance coalitions. As the number of actors in the system increases, it becomes more difficult to predict the outcome of an armed conflict since state leaders must correctly predict the behavior of many independent actors. For example, the opposing incentives arising from the problems of collective action and entrapment complicate decision makers' ability reliably to predict which actors will support or oppose them (Christensen and Snyder 1990; Snyder 1984; Waltz 1979). As a result, decision makers will be less confident in predicting international responses to their escalation of a conflict. Combining these variables with the risk propensities of national leaders, we develop the following three hypotheses:

HYPOTHESIS 1. *In a multipolar system, a risk-acceptant decision maker will be more likely to escalate a militarized dispute than in a bipolar system. In contrast, in a multipolar system a risk-averse decision maker will be less likely to escalate a conflict than in a bipolar system.*[11]

HYPOTHESIS 2. *As the number of great powers increases, risk-acceptant decision makers will be more likely to escalate a militarized dispute. In contrast, as the number of great powers increases, risk-averse decision makers will be less likely to escalate a conflict.*

HYPOTHESIS 3. *As the number of great-power alliance coalitions increases, risk-acceptant decision makers will be more likely to escalate a militarized dispute. In contrast, as the number of great-power alliance coalitions increases, risk-averse decision makers will be less likely to escalate a conflict.*

Next, we consider the conceptions of system structure that focus on the distribution of capabilities among individual great powers and among great-power alliance coalitions. As military capabilities become more evenly distributed, decision makers become less certain of their predictions regarding the outcome of an armed conflict because these outcomes are highly sensitive to errors in estimating the response of other actors. Given any number of great powers, an equal distribution of capabilities creates more uncertainty than does a skewed one. For example, if there are two actors, an even distribution (50–50) leaves greater uncertainty about the outcome of a conflict than does a more unequal (70–30) distribution. Similarly, if there are four actors, an equal division of capabilities (25–25–25–25) creates more uncertainty than a less even (55–15–15–15) division. In the latter case, one need only predict the actions of the dominant power in order to project the outcome of an armed confrontation. In the first case, however, mispredicting the behavior of any of the great powers could have significant consequences. Combining this argument with the intervening effects of risk propensity produces the following hypotheses:

HYPOTHESIS 4. *As capabilities become more evenly distributed among the great powers, risk-acceptant decision makers will be more likely to escalate a militarized dispute. In contrast, as capabilities become more evenly distributed among the great powers, risk-averse decision makers will be less likely to escalate a conflict.*

HYPOTHESIS 5. *As capabilities become more evenly distributed among great-power alliance coalitions, risk-acceptant decision makers will be more likely to escalate a militarized dispute. In contrast, as capabilities become more evenly distributed among great-power alliance coalitions, risk-averse decision makers will be less likely to escalate a conflict.*

Finally, we consider the conception of system structure that includes the extent of alliance ties crossing coalition boundaries. As the number of cross-cutting ties in the system increases, it becomes more difficult to predict other states' behavior. The more links a particular state has with an opposing alliance coalition, the lower the reliability of its support for members of its own coalition, because its security interests are divided between coalitions. Combining this variable with risk propensity produces our final structural-realist hypothesis:

HYPOTHESIS 6. *As the extent of cross-cutting ties between great-power alliance coalitions increases, risk-acceptant decision makers will be more likely to escalate a militarized dispute. In contrast, as the extent of cross-cutting ties increases, risk-averse decision makers will be less likely to escalate a conflict.*

In previous work, we have found that system uncertainty does appear to affect the *initiation* of militarized disputes in the manner discussed (Huth, Bennett, and Gelpi 1992). We believe, however, that this effect will be attenuated with regard to the *escalation* of disputes to war. When decision makers are considering the initiation of a militarized dispute, many states have not taken any public position with regard to the particular challenge that may be offered. Although leaders have some general idea of who is likely to align with whom, it remains relatively unclear whether states not directly involved in the challenge will take an active role in the dispute. Consequently, the level of system uncertainty is likely to be salient to national leaders in such situations. When deciding whether to *escalate* a dispute, however, decision makers are generally able to gather a number of *behavioral* cues that reduce the influence of system uncertainty. Once a challenge has been issued, all great powers, including those not directly targeted, must choose some sort of response to this action, even if they choose to remain uninvolved. As a result, leaders of the challenging state will have a much clearer conception of who is likely to oppose them in case of an armed conflict. States that fail to respond with any counterthreats may be disregarded. The challenger can therefore focus its attention on the states that have threatened to resist with force. Thus structural variables such as the number of great powers and the distribution of capabilities among them may be less relevant to state decisions once leaders are more certain of who is willing to use force to resolve a particular dispute. One implication of this argument is that although we previously found systemic factors to be an important influence on dispute initiation, we should not be surprised to find the substantive and statistical impact of the systemic variables discussed in hypothesis 1–6 to be reduced in this study.

Hypotheses from Rational Deterrence Theory

Consistent with the realist framework, rational deterrence theory also argues that the challenger weighs the costs and benefits of escalating the dispute to the point of armed conflict versus accepting the status quo (e.g., Bueno de Mesquita 1981; Ellsberg 1961; George and Smoke 1974; Jervis 1984; Powell 1990; Schelling 1960, 1966; Snyder 1961; Wagner 1982; Wu 1990; Zagare 1987). In order for deterrence to succeed, the challenger's expected utility for accepting the status quo must be greater than its expected utility for attempting to overturn the status quo through the use of force. This condition is commonly represented in the deterrence literature by the following relationship:

$$\text{Value of status quo} > \text{P (win)} * \text{value of victory} + [1 - \text{P (win)}] * \text{value of defeat.}$$

Deterrence theory argues that the credibility of threats is the primary determinant of deterrence success or failure: the more credible the threat, the more likely deterrence will succeed. A credible threat implies that the deterring party has the military capabilities to impose high costs on a challenger and that the challenger perceives that the deterring party is willing to do so. As a result, credibility is a function of two central variables: the balance of military capabilities between challenger and defender and the challenger and defender's level of resolve. The balance of capabilities influences the challenger's probability of victory as well as the value it places on a victory or defeat on the battlefield. As the balance of military capabilities shifts toward the challenger, it becomes more likely that it will be able to prevail in an armed conflict. Additionally, under these circumstances the costs of armed conflict decline, increasing the net utility of a victory on the battlefield. The challenger and defender's resolve is a function of the value placed on victory, defeat, and the status quo. For example, as the challenger places a larger value on victory relative to the value of the status quo, it becomes more resolved to use force.

Deterrence theory has been developed by many scholars over the past several decades. Each of these scholars emphasizes some of their own particular aspects of direct or extended deterrent situations, but at the core of these alternative theory-building efforts is a common set of explanatory variables. While there is value in developing more precise formulations of the deterrence model, our purpose here is to test the general explanatory power of the deterrence approach relative to that of structural realism. Thus we restrict our attention to a set of core variables in a deterrence model. Drawing heavily on the deterrence literature, we have identified five common variables: (1) the balance of conventional military capabilities, (2) the defender's possession of nuclear weapons, (3) challenger's and defender's interests at stake in the dispute, (4) challenger's and defender's involvement in other disputes, and (5) the past dispute behavior of challenger and defender. We now derive hypotheses regarding the effects of each of these variables by relating them to the challenger's expected utility calculation whether to escalate a militarized dispute.

Balance of Conventional Military Capabilities. The balance of military forces between challenger and defender is one of the most basic components of the deterrence approach. Analysts may disagree about how to measure the balance or how strong the effects of the balance may be relative to other factors, but without the inclusion of the balance of capabilities, it is hard to conceive of a model as a "deterrence" model.[12] As the conventional military capabilities of the challenger improve relative to those of the target, the costs of armed conflict decrease and the probability of victory increases. As a result, the challenger may attempt to exploit that military advantage either to coerce the defender into making concessions or to impose a change in policy through victory on the battlefield. These capabilities must take into account both the probable support or opposition of other states involved in the dispute and those states' distance from the location of the dispute.

HYPOTHESIS 7. *The more favorable the balance of military capabilities for the challenger, the higher the probability that it will escalate a militarized dispute against the defender.*

Defender's Possession of Nuclear Weapons. The literature on the deterrent value of nuclear weapons is enormous and draws many nuanced distinctions between the effects of various nuclear environments and strategies (Glaser 1990). Perhaps the central assertion that can be drawn from this literature, however, is that the possession of a secure second-strike capability by the defender drives the value that the challenger places on defeat down sharply, increasing the likelihood of deterrence success (e.g., Glaser 1990; Jervis 1984; Zagare 1987). Additionally, the use of nuclear weapons may also decrease the challenger's likelihood of success on the battlefield, which also increases the prospects for deterrence success.

HYPOTHESIS 8. *If the defender possesses a second-strike nuclear capability, the probability that the challenger will escalate a militarized dispute against the defender will decrease.*[13]

Interests at Stake for Challenger and Defender. As the interests at stake in a dispute increase for each of the two parties, the value that they place on prevailing in an armed conflict increases as well. Additionally, as the challenger's and defender's interests at stake in the crisis increase, the relative value that the challenger places on the status quo will decrease, while the defender's utility for the status quo will increase (Betts 1987; George and Smoke 1974; Jervis 1984; Morgan 1990; Morrow 1989). Geographically proximate states are of strategic value since they can act as defensive buffers against external security threats or can be used as bases from which to project military power for offensive purposes. Additionally, in the nineteenth and early twentieth centuries, state elites often considered colonial territories to be of vital importance. Thus states will be more willing to use force in disputes concerned with these vital areas. The hypothesis regarding the challenger's interests at stake, therefore, is as follows:

HYPOTHESIS 9. *When a militarized dispute involves the control of territory that the challenger considers to be part of its national or colonial territory or a state adjacent to that territory, the challenger will be more likely to escalate the dispute.*

A challenger will also be aware that this same logic applies to the behavior of the target state. As a result, the challenger will realize that the target is highly motivated to defend areas near its own vital interests. The hypothesis regarding the defender's interests at stake, therefore, is as follows:

HYPOTHESIS 10. *When a militarized dispute involves the control of territory that the target considers to be part of its national or colonial territory or a state adjacent to that territory, the challenger will be less likely to escalate the dispute.*

Past Behavior of Challenger and Defender. In deterrence theory the bargaining reputation of states is of central concern. A state's determination to stand firm in a crisis is a function both of contextual factors (e.g., the balance of forces) and the state's independent willingness to use force if necessary. A state's reputation for resolve captures this independent willingness to stand firm. If a state backs down during a crisis, it does so knowing that it is doing damage to its bargaining reputation. Since states have no strategic incentive to back down in crises (i.e. there is no reason to "bluff" capitulation), backing down sends a reliable and revealing message that the state is, in fact, weakly resolved.[14] As a result, state leaders should perceive the defeat of their adversary in a previous dispute as an indication of general weakness on their part (Nalebuff 1991; Powell 1990; Schelling 1966). Challengers should conclude that the time is now favorable for confronting their opponent and resolving any dispute to their advantage. The defender, of course, should draw similar inferences about the resolve of the challenger based on its previous dispute behavior.

Hypothesis 11. *If the defender backed down in a previous dispute, the likelihood that the challenger will escalate the current dispute will increase.*

Hypothesis 12. *If the challenger backed down in a previous dispute, the likelihood that the challenger will escalate the current dispute will decrease.*

Current Dispute Behavior of Challenger and Defender. Finally, one variable that we believe should be an important part of any deterrence model is the current dispute involvement of the parties. This variable has received little attention in the traditional deterrence literature—perhaps because it yields few policy implications for the behavior of the defender. Yet involvement in other conflicts clearly has a direct impact on the diplomatic and military resources that a state can direct to the dispute at hand. As a result, this variable should be as much of a basic component of a deterrence model as the other variables we have discussed. If a state's diplomatic and military resources are committed to one dispute, then it is less likely that the state will be in a favorable position to prevail in additional disputes with other states. This logic applies to both the challenger and the defender.

Hypothesis 13. *Challenger involvement in a militarized dispute or war with a third state leads to a lower probability that the challenger will escalate a militarized dispute against the defender.*

Hypothesis 14. *Defender involvement in a militarized dispute or war with a third state leads to a higher probability that the challenger will escalate a militarized dispute against the defender.*

RESEARCH DESIGN AND MEASUREMENT OF VARIABLES

Population of Cases

We will test our hypotheses on the population of great-power extended and direct immediate deterrence encounters from 1816 to 1984. Following Small and Singer (1982) and Levy (1983) we identify the great powers between 1816 and 1984 as follows:

Great Britain 1816–1945
France 1816–1940
Russia/Soviet Union 1816–1984
Austria-Hungary 1816–1918
Prussia/Germany 1816–1945
Italy 1860–1943
United States 1899–1984
Japan 1895–1945
China 1950–1984

A great-power deterrence encounter is defined by the explicit verbal threat of force or the movement and buildup of military forces in preparation for armed conflict by a challenging great power and a counterthreat by the defending great power (Huth 1988). A challenger initiates a dispute by coupling a demand with a threat of force, which is then resisted with a retaliatory threat by a great-power target. In a direct immediate deterrence case, the challenger's threat is targeted at the homeland of the great-power defender, whereas in extended immediate deterrence cases the initial threat of the challenger is aimed at a minor power.[15] The list of cases is presented in the Appendix.[16]

Measurement of Variables

Dispute Escalation. We define dispute escalation as the failure of the deterrent policies of the great-power defender (Huth 1988). Deterrence may fail in one of two ways: the challenger may resort to the large-scale use of military force, or the defender may capitulate to the demands of the challenger regarding the central issues at stake in the dispute under the threat of armed conflict.[17] Deterrence success, on the other hand, is defined as the challenger's failure to use force combined with its inability to coerce the defender to make concessions on the essential issues at stake in the dispute.[18] The codings for the individual cases of deterrence success and failure are listed in the Appendix.

System Uncertainty. As discussed earlier, we use six different indicators to operationalize system uncertainty: (1) bipolar versus multipolar systems, (2) the number of great powers, (3) the diffusion of military capabilities among the great powers, (4) the number of great-power alliance clusters, (5) the diffusion of capabilities across clusters, (6) the level of alliance ties that cross cluster boundaries. We have coded each of these variables so that larger values correspond to higher levels of system uncertainty. We shall limit ourselves here to a relatively brief description of the coding procedures. (For a detailed discussion of each of the operational indicators, see Huth, Bennett, and Gelpi 1992.)

1. *Bipolarity versus multipolarity.* This indicator is coded as a dummy variable. It is coded 1 when Waltz (1979) identifies the system as multipolar (1816–1945), and 0 when he argues that it is bipolar (1946–84).
2. *Number of great powers.* The number of great powers in the international system is derived from the list just given.
3. *Diffusion of capabilities across great powers.* The military capabilities of each state are measured by averaging total manpower, military spending, and spending per soldier as a percentage of the total capabilities of the great powers during each year in which there is a dispute (spending per soldier is taken as a very rough measure of the quality of the troops).[19] We then use an index in order to measure how concentrated these aggregate capabilities are among the great powers (Singer, Bremer, and Stuckey 1972). An index score of 1 reflects maximum concentration of capabilities, while 0 reflects an even distribution. We then convert the concen-

tration of capabilities into the diffusion of capabilities by setting diffusion equal to 1 minus the concentration index, so that a higher value corresponds to greater system uncertainty.

4. *Number of great-power alliance clusters.* Drawing on Bueno de Mesquita (1975), we use τ_b (an ordinal measure of association varying from -1, indicating an opposite pattern, to 1, indicating complete similarity) to measure the similarity of alliance commitments between the great powers. These τ_b "similarity scores" are taken to reflect the level of shared interests between states. We then use typal analysis (McQuitty 1957) to group states into clusters based on this alliance similarity. As a result, alliance clusters are characterized by states sharing relatively high τ_b similarity scores with one another. That is, they tend to share alliance ties with one another and/or with common third states.
5. *Diffusion of capabilities across clusters.* We also utilize the converted concentration index in order to determine the diffusion of capabilities among alliance clusters. Since it is not certain that an ally will come to the aid of one of its partners in a crisis, we discount the aggregate capabilities of a cluster by the level of alliance-similarity scores within that cluster, which we take to reflect shared interests. As a result, each cluster's capabilities are discounted by the level of alliance tightness among its members. The tightness of a cluster is defined as the average of the similarity scores of each pair of its members (Bueno de Mesquita 1975).
6. *Cross-cutting alliance ties between clusters.* Our measure of cross-cutting alliance ties is calculated as the average of all intercluster alliance-pattern similarity scores. However, this variable ranges from -1 to 1, making its interpretation in interaction with risk propensity difficult to sort out. As a result, we add a constant of 1 to simplify the interpretation of this variable.

In testing for the robustness of our results concerning system uncertainty, we shall include these various measures in differing combinations with one another. In order to test a comprehensive model of system uncertainty that includes the broadest range of indicators, high levels of multicolinearity forced us to combine indicators two through six into a composite measure of system uncertainty through the use of factor analysis.[20] The use of factor analysis is quite appropriate in this case because our separate indicators are all measuring some aspect of a single concept—system uncertainty. System uncertainty factor 1 (system size) is composed primarily of the number of major powers in the system, the number of alliance clusters, and the average level of alliance similarity across these clusters. System uncertainty factor 2 (capability diffusion), on the other hand, is composed almost exclusively of the concentration of capabilities across great powers and alliance clusters.

Risk Propensity. Our measure of risk propensity includes both individual and situational components. We identify three sources of risk acceptance, and if *any two* are present, we code the decision makers in that state as risk-acceptant. Because of the limited data available for coding this variable, we believe that it would be a mistake either to code the variable on the basis of a single indicator or require that all three indicators converge.

Our three indicators of risk propensity are indirect in the sense that they focus empirically on states' relative capabilities, alliance behavior, and domestic unrest. This may raise some concerns because we move from such broad measures to the attitudes of individual decision makers (Levy 1992). It is important to remember, however, that we are working with the realist assumption that states behave as unitary rational actors. Consequently, we do not face the problem of aggregating the differing risk propensities of various decision makers within a state. Without doubt, this unitary actor receives advice from many sources. However, we assume that the final decision regarding conflict escalation is the responsibility of a single dominant leader. Additionally, we assume that this decision maker's risk propensity is shaped by his or her role as a domestic political leader seeking to maintain a hold on office and as the decision maker ultimately responsible for his or her country's national security. As a result, our indicators of risk propensity attempt to tap a state's external security environment and the leader's level of domestic political support.

We realize fully that these indicators are crude and indirect at best. Nonetheless, similar measures have been used successfully in empirical research in international politics. Bueno de Mesquita (1981a, 1985, 1992), for example, has effectively used his indicator in several different works. Indicators derived from prospect theory have been fruitfully applied, as well (Farnham 1992; McDermott 1992; McInerney 1992). Finally, we have successfully used our indicator of risk propensities in previous work on great-power conflict initiation (Huth, Bennett, and Gelpi 1992).

The first aspect of risk propensity that we identify is Bueno de Mesquita's (1981) measure of individual risk propensity, which is calculated using a state's expected utility for conflict. If a state has a positive expected utility against fewer great powers than have positive utility against it, it is coded as 1 (risk-acceptant) and 0 otherwise.[21] Our second and third risk factors emerge from prospect theory, which argues that decision makers who perceive themselves to be in the domain of losses are likely to be risk-acceptant (Kahneman and Tversky 1979; Quattrone and Tversky 1988). We argue that decision makers will perceive themselves to be in the domain of losses or gains based on two variables—the relative industrial–military position of their country compared to the defender and the domestic political conditions within their country. The four indicators we use to capture these two variables are as follows:

1a. *Relative industrial growth.* An index of production incorporating industries that provide a critical base for military capabilities has been constructed for each great power.[22] We calculated three-year moving averages, and if the challenger's moving-average growth rate is lower than the defender's, then a value of 1 is coded, and 0 otherwise.
1b. *Relative military growth.* Three-year moving averages have been calculated for the annual growth of manpower, military spending, and spending per soldier. If the challenger's moving-average figure is lower than the defender's, a value of 1 is coded, and 0 otherwise.
2a. *Economic growth.* Our measure of economic growth compares the challenger's current growth rate with a three-year moving average. If the current year's rate is 50% or more below the moving average, then a value of 1 was coded, and 0 otherwise.
2b. *Strike activity.* The challenger's current number of strikes and labor days lost are compared to a three-year moving average. If either the number of strikes or the labor days lost due to strikes in the current year is 50% higher than the moving average, then a 1 is coded, and 0 otherwise.

Once again, in order to be coded as risk-acceptant, a challenger must meet any two of three conditions: (1) a low expected utility for conflict, (2) poor military or industrial growth relative to the *defender*, and (3) low levels of economic growth or high levels of strike activity relative to *its own* recent levels. We interact this indicator of risk propensity with each of the various indicators of system structure. The coefficients on the system uncertainty measures alone represent the effects of the system on risk-averse actors, while the coefficients on the system uncertainty measures interacted with risk propensity represent the additional effects of the system on risk-acceptant states. So as not to constrain all actors in the system to have an equal probability of conflict initiation at the lowest level of system uncertainty, we also include risk propensity by itself. Our expectation would be that this variable will have a positive coefficient.

Balance of Conventional Military Capabilities. This variable is a ratio comparing the capabilities of the challenger(s) to the total capabilities of the challenger(s) and defender(s), as identified in the Appendix. Individual states' capabilities are measured as discussed previously, with the addition that they are discounted for the distance either to the point of dispute or to the territory of the nearest opposing great power, whichever distance is shorter. The effect of distance on states' ability to project military power is calculated as described by Bueno de Mesquita (1981a).

Defender Possession of Second Strike Nuclear Capability. This variable is coded 1 if the defender possesses the capability to deliver nuclear weapons onto the population of the challenger following the absorption of a nuclear first strike, and 0 otherwise (Arkin and Fieldhouse 1985; Betts 1987).

Interests at Stake for Challenger and Defender. For the defender, this variable is given a value of 1 if the issues at stake in the dispute centered on the control or acquisition of territory adjacent to, or part of, the homeland or colonial empire of the defender. Similarly, for the challenger, we code 1 if the issues at stake in the dispute centered on the control or acquisition of territory adjacent to, or part of, the homeland or colonial empire of the challenger (see Huth, Bennett, and Gelpi 1992).

Past Behavior of Challenger and Defender. The coding procedure for this variable is identical for challenger and defender. In each case, this variable is coded 1 if the state suffered a diplomatic put-down in a dispute with the same opponent within the past 10 years (see Huth 1988).

Current Dispute Involvement of Challenger and Defender. Once again, the coding procedure for this variable is identical for challenger and defender. In each case, this variable was coded 1 if the state was either involved in at least one other militarized dispute within the past six months, or a war with a third party during the current or previous year.[23] The Correlates of War data sets on international and extrasystemic wars and militarized disputes were used to identify involvement in disputes and wars.

DATA ANALYSIS

In order to determine the robustness of our results, we present equations which use five differing operationalizations of system uncertainty, ranging from the narrowest definition of this concept to the most comprehensive. The first equation follows Waltz's conception of system structure; the second focuses on the number of great powers; the third adds the distribution of capabilities among great powers; the fourth shifts its focus to alliance clusters and includes both the number of clusters and the distribution of capabilities among them; and the fifth is a composite of all of these indicators combined in our two factor scores, namely, system size and capability diffusion.[24] In each equation, however, our measurement of the deterrence model remained constant. The results of the probit analysis are presented in Table 1.[25]

The general conclusion that we draw from the results is that rational deterrence theory provides a much more compelling explanation of great-power escalatory behavior than does structural realism. None of the variables from any of the different specifications of the structural realist model are statistically significant in the expected direction. In contrast, seven of the eight estimated coefficients from the deterrence model are in the expected direction in every equation and are statistically significant.

Across the various specifications of the equation,

TABLE 1

Probit Estimates of Effects on Deterrence Outcome across Various Models

VARIABLES	MODEL1	MODEL 2	MODEL 3	MODEL 4	MODEL 5
Constant	−.73 (1.07)	−.95 (1.24)	.35 (3.15)	−.59 (2.32)	−.71 (1.37)
Structural realism					
Multipolar	−.09 (.81)	—	—	—	—
Multipolar × risk-acceptant	−1.79 (.82)**	—	—	—	—
Number of GPs	—	.008 (.16)	.02 (.16)	—	—
Number of GPs × risk	—	−.48 (.20)***	−.51 (.20)***	—	—
Capability diffusion over GPs	—	—	−1.72 (3.86)	—	—
Cap. diff. over GPs × risk	—	—	4.04 (5.08)	—	—
Number of alliance clusters	—	—	—	.33 (.24)*	—
Number of clusters × risk	—	—	—	−.70 (.29)***	—
Cap. diff. over clusters	—	—	—	−2.14 (2.39)	—
Cap. diff. over clusters × risk	—	—	—	1.91 (3.28)	—
System uncertainty 1 (Size)	—	—	—	—	.21 (.32)
System size × risk	—	—	—	—	−.97 (.36)***
System uncertainty 2 (diffusion)	—	—	—	—	−.20 (.24)
System diffusion × risk	—	—	—	—	.18 (.32)
Risk-acceptant	1.38 (.74)**	2.81 (1.23)***	−.18 (3.93)	1.50 (3.27)	1.55 (1.54)
Deterrence theory					
Balance of forces	1.59 (.89)**	1.72 (.90)**	1.69 (.92)**	1.53 (.90)**	1.73 (.94)**
Secure 2d strike	−2.58 (.92)****	−2.59 (.77)****	−2.60 (.78)****	−1.77 (.61)****	−2.33 (.83)****
Defender vital interests	−1.14 (.42)****	−1.30 (.45)****	−1.34 (.46)****	−1.22 (.45)****	−1.29 (.46)****
Challenger vital interests	1.08 (.42)****	1.09 (.42)****	1.09 (.43)****	0.93 (.41)***	1.09 (.44)****
Defender backed down	1.09 (.43)****	1.37 (.47)****	1.36 (.47)****	1.14 (.44)****	1.23 (.46)****
Challenger backed down	−.62 (.54)*	−.66 (.55)*	−.63 (.57)*	−.81 (.56)*	−.72 (.57)*
Defender other dispute	.75 (.39)**	.92 (.39)***	.94 (.39)****	.99 (.41)***	.96 (.42)***
Challenger other dispute	.03 (.37)	−.004 (.36)	−.02 (.37)	−.05 (.41)	.05 (.41)
Percentage correct predictions	73	73	72	74	76

Notes: n = 97. Standard errors are in parentheses. Significance tests are one-tailed except for the structural-realist variables interacted with *Risk-acceptant*, which are two-tailed.
*$p < .15$.
**$p < .05$.
***$p < .025$.
****$p < .01$.

the predictive power of the probit model remains quite constant, with approximately 74% of the cases correctly predicted. For example, model 5 correctly predicts 40 out of 52 deterrence successes, and 34 out of 45 deterrence failures. These predictions yield an overall success rate of 76%.

If we look at the results in Table 1 concerning the five differing specifications of system structure, hypotheses 1–6 would have predicted negative coefficients on the system uncertainty factors alone and positive coefficients of greater absolute value on the system uncertainty factors interacted with risk propensity. Neither of these patterns emerges. Generally, variables measuring the number of actors (states or alliance coalitions) in the system are insignificant for risk-averse decision makers and are in the wrong direction for risk-acceptant decision makers. Variables measuring the diffusion of capabilities among great power actors are generally in the expected direction, but do not approach substantive or statistical significance.[26] Finally, the risk propensity variable by itself is generally not statistically significant, though it has the predicted positive coefficient. These weak results for the systemic variables are consistent with our earlier argument that the uncertainty created by the international system may have a limited impact on great-power militarized dispute escalation since other great powers' responses to the initiation of a dispute will reduce the challenger's uncertainty about the identity of its allies and adversaries.

We now turn to the findings concerning the variables in the rational deterrence model. Since the marginal effects of these variables differ little across the various equations, we draw our examples of the marginal effects from model 5, the most comprehensive specification of system uncertainty. First, the

TABLE 2

Marginal Impact of Variables from the Deterrence Model on the Probability of Dispute Escalation

CHANGE IN VALUES OF INDEPENDENT VARIABLE	CHANGE IN PROBABILITY OF DISPUTE ESCALATION (%)
Conventional forces	
Challenger:defender ratio of conventional capabilities	
From 1:3 to 1:2	5.3
From 1:2 to 1:1	11.6
From 1:1 to 2:1	11.4
From 2:1 to 3:1	5.0
Nuclear weapons	
Does the defender possess a secure second-strike Capability? (*no* to *yes*)	−50.9
Interests at stake	
Are the defender's proximate territorial interests at stake? (*no* to *yes*)	−41.2
Are the challenger's proximate territorial interests at stake? (*no* to *yes*)	35.3
Previous dispute behavior	
Did the defender capitulate in its previous dispute with the challenger? (*no* to *yes*)	38.0
Did the challenger capitulate in its previous dispute with the defender? (*no* to *yes*)	−26.8
Other dispute involvement	
Was the defender involved in another militarized dispute or war? (*no* to *yes*)	32.5

Note: The changes in the probability of dispute initiation are calculated from the coefficients in the equation in Table 1 by changing the value of a single variable while holding all other continuous variables in the model at their mean values and all dummy variables at zero. The change in position on the cumulative standard normal distribution is then converted into the percentage change in the probability of a deterrence success.

conventional balance of forces has a significant effect on dispute escalation, and nuclear weapons have an even stronger impact. As Table 2 illustrates, a change in the conventional balance from a three-to-one defender advantage to a three-to-one challenger advantage increases the probability of escalation by approximately 33%.[27] In all five equations, the defender's possession of a second-strike capability has a powerful deterrent effect on the escalatory behavior of the challenger, and in all five of the equations this effect is statistically significant. As a result, we are able to distinguish between the deterrent effects of bipolarity and nuclear weapons during the postwar period, and our results suggest that nuclear weapons have had a much greater impact. The defender's possession of a secure second-strike reduces the likelihood of escalation by 51%. This percentage change represents by far the single largest marginal effect in the entire equation. The defender's possession of a second-strike capability does not by itself ensure deterrence success, but it makes a very large contribution toward this outcome.

These results contrast with our previous finding that nuclear weapons do not deter the *initiation* of great-power militarized disputes (Huth, Bennett, and Gelpi 1992). We believe that these differing findings reflect the fact that challengers may be more uncertain about whether a nuclear defender will respond to their initiatives. As a result, they may begin a dispute in order to probe the resolve of the nuclear target, knowing that they can back away from escalation rather than risk nuclear conflict if the defender demonstrates sufficient resolve.

The interests at stake for both challenger and defender are statistically significant and in their expected directions. If a dispute centers on the control or acquisition of territory that is a part of or bordering on the homeland or colonial empire of the defender, the likelihood of challenger escalation is reduced by 41% (Table 2). At the same time, if the dispute centers on territorial issues proximate to the challenger, the likelihood of escalation increases by 35%. These findings confirm our expectation that dispute escalation is not solely a function of conventional or nuclear capabilities. Intrinsic interests at stake also play a role in determining the outcome of a dispute (e.g., George and Smoke 1974; Jervis 1984).

As hypothesized, previous capitulation by either the challenger or defender appears to be a sign of general weakness on their part. If the defender was forced to back down in a previous dispute with the challenger, the likelihood that the challenger will escalate the current dispute increases by almost 38% (Table 2). Similarly, if the defender forced the challenger to capitulate in a previous dispute, then the likelihood that the challenger will escalate the current dispute decreases by 27%. The statistical significance of this result, however, is somewhat marginal ($p <$ .11). We believe that this weaker result is due to selection effects which attenuate this coefficient, since some other factors must have induced a previously irresolute challenger to initiate the current dispute.[27]

Our general findings concerning previous dispute behavior support those who emphasize the importance of developing a reputation for toughness with a particular adversary in order to make deterrent threats against them effective in future encounters (e.g., Schelling 1966). We must emphasize, however, that this result does *not* imply that the outcome of previous disputes with third parties has strong reputational effects (see also Hopf 1990; Huth 1988; Snyder and Diesing 1977).

Finally, as hypothesized, we find that the defender's current involvement in other disputes has a positive and significant effect on the likelihood of escalation. This finding suggests that the challenger views the defender's preoccupation with other conflicts as a favorable opportunity for achieving its goals. If the defender is involved in another conflict, the probability of the challenger escalating its dispute with the target increases by 32% (Table 2). The effect of the challenger's other dispute involvement, however, is not statistically or substantively significant. One possible explanation of this result is that challengers who initiate a dispute against the target despite involvement in other conflicts must be highly resolved to prevail, or they would not have confronted the target to begin with. Consequently, one might expect to find that other dispute involvement affects the challenger's likelihood of initiating a dispute but not their likelihood of escalating one. This explanation is consistent with our previous finding that the challenger's other dispute involvement leads to a lower likelihood of dispute initiation (Huth, Bennett, and Gelpi 1992).

CONCLUSION

Our central conclusion is that rational deterrence theory provides a much more compelling explanation of great-power decisions to escalate militarized disputes than does structural realism. None of the variables from any of the specifications of the structural realist model were statistically significant in the hypothesized direction. Structural theories may provide some insight into the initiation of great-power disputes (Huth, Bennett, and Gelpi 1992), but they do not explain decisions by state leaders to escalate such disputes. In line with the converging body of evidence in support of rational deterrence theory, we find that the variables specified in this model have a consistently significant effect on great-power dispute escalation.

Our results do not offer any support to the logic underlying the Waltzian argument that uncertainty in the international system leads to more conflict (Waltz 1979). As a result, we believe that arguments that draw on Waltz to conclude that emerging multipolarity in Europe represents a significant threat to peace (e.g., Mearsheimer 1990) are overstated. Our previous results suggest that an increase in system uncertainty should not generally lead to an increased incidence of dispute initiation in the international system. Multipolarity would increase the frequency of dispute initiation only if risk-acceptant state leaders greatly outnumber risk-averse leaders. Additionally, in this study we find no evidence that multipolarity will lead to an increased incidence of escalation of those disputes regardless of the risk propensities of decision makers.

The prospects for peace in post–Cold War Europe, and other areas of the globe where great-power interests may conflict, may depend more importantly on the maintenance of credible deterrent policies. The post–Cold War environment has encouraged many policy makers to press for far-reaching reductions in nuclear arsenals. However, our findings suggest that the possession of a second-strike capability has an important deterrent effect. Consequently, great powers such as the United States should not allow their pursuit of the peace dividend to undermine the potency of their nuclear deterrent.

APPENDIX

Table A-1 (on page 620) lists the population of extended and direct-immediate-deterrence encounters among great powers between 1816 and 1984. In total, we identified 97 cases. For each case, we list the great power challenger and defender states, as well as minor power targets when appropriate. In addition, we include our coding on the dependent variable—success or failure of deterrence. The operational criteria for selecting the cases and the coding of the dependent variable have been presented.

TABLE A-1

Deterrence Encounters among Great Powers, 1816–1984

YEARS	GREAT-POWER CHALLENGERS	GREAT-POWER DEFENDERS	MINOR-POWER TARGET	DETERRENCE SUCCESS OR FAILURE[b]
1832–33	UK, France	Prussia	Netherland	F
1833	UK, France	Russia	Turkey	S
1840	France	UK, Russia, Prussia A–H	Turkey	S
1848–49	Prussia	UK, Russia	Denmark	F
1848–49	Russia	UK, France	Turkey	F
1850	Prussia	A–H Russia, France	—	S
1853–56	Russia	UK, France, A–H	Turkey	F
1856–57	Prussia	France, A–H	Switzerland	S
1859	France	A–H, Prussia	—	F
1860–61	France	A–H, Italy	—	S
1861	Russia	UK	Japan	S
1864	Prussia	A–H	Saxony	S
1865	A–H	Prussia	—	S
1866	Prussia, Italy	A–H	—	F
1870	Prussia	France	—	F
1876	UK	Russia	Turkey	F
1877	Russia	UK	Turkey	F
1885	Russia	UK	Afghanistan	S
1893	France	UK	Siam	S
1895	Russia, France, Germany	Japan	—	F
1897	Germany	Russia	China	F
1897–98	Russia	UK, Japan	China	F
1898	France	UK	Egyptian Sudan	S
1899–1900	Russia	Japan	Korea	S
1902–3	Germany, UK, Italy	U.S.	Venezuela	S
1903–5	Japan	Russia	Korea	F
1904	UK	Russia	—	F
1905–6	Germany	France, UK	—	S
1908–9	A–H, Germany	Russia	Serbia	F
1911	Germany	France, UK	—	S
1912–13	A–H, Germany	Russia	Serbia	F
1914–18	A–H, Germany	Russia, France, UK	Serbia, Belgium	F
1914	Japan	Germany	China	F
1915	Japan	Russia	China	F
1915	Germany	U.S.	—	S
1917–20	UK, France, U.S., Italy, Japan	USSR	—	F
1918–19	USSR	UK	Estonia	S
1919–20	USSR	UK, Germany	Latvia	F
1919	Germany	UK	Latvia	S
1920	France	Germany	—	F
1920–22	Japan	USSR	—	F
1920–21	USSR	UK	Persia	F
1920	UK	USSR	—	S
1921	France, UK, Italy	Germany	—	F
1922	Italy	UK	Greece	S
1923	France	Germany	—	F
1931–33	Japan	USSR	China	F
1932	UK, U.S.	Japan	—	S
1933–35	USSR	Japan	Manchukuo	S
1934–36	Italy	UK	Ethiopia	F
1935–36	Japan	USSR	Outer Mongolia	S
1936	Germany	France	—	F
1937	USSR	Japan	Manchukuo	S
1938	USSR	Japan	Manchukuo	F
1938	Germany	UK, France, USSR	Czechoslovakia	F
1938–39	Italy	France, UK	Tunisia	S
1938–40	Japan	UK, France	China	F

TABLE A-1

Deterrence Encounters among Great Powers, 1816–1984 — (Continued)

YEARS	GREAT-POWER CHALLENGERS	GREAT-POWER DEFENDERS	MINOR-POWER TARGET	DETERRENCE SUCCESS OR FAILURE[b]
1939	Germany	France, USSR	Czechoslovakia	F
1939	Japan	USSR	Outer Mongolia	F
1939–45	Germany	UK, France	Poland	F
1940–41	USSR	Germany	Finland	S
1941–45	Germany	USSR	—	F
1941–45	Japan	U.S.	—	F
1945–46	USSR	U.S.	Turkey	S
1946	USSR	U.S.	Iran	S
1948–49	USSR	U.S.	—	S
1949–51	USSR	U.S.	Yugoslavia	S
1950	China	U.S.	Taiwan	S
1950–53	U.S.	China	N. Korea	F
1954–55	China	U.S.	Taiwan	S
1957	U.S.	USSR	Syria	S
1958	China	U.S.	Taiwan	S
1958	U.S.	USSR	—	F
1958–59	USSR	U.S.	—	S
1960–61	USSR	U.S.	Congo	S
1961	U.S.	USSR	Cuba	S
1961	U.S.	USSR	Cuba	S
1961	USSR	U.S.	—	S
1962	China	USSR	—	S
1962	U.S.	USSR	Cuba	F
1964–65	USSR	China	—	S
1965	U.S.	China	N. Vietnam	F
1966–68	China	USSR	—	S
1969	China	USSR	—	S
1970	U.S.	USSR	Syria	F
1973	USSR	U.S.	Israel	S
1974	China	USSR	Mongolia	S
1977	USSR	China	—	S
1978	China	USSR	—	S
1978–79	U.S.	USSR	Iran	S
1978–79	China	USSR	Vietnam	F
1979	U.S.	USSR	Cuba	S
1979–80	USSR	China	—	S
1980	China	USSR	—	S
1983	U.S.	USSR	Iran	S
1983–84	USSR	U.S.	—	S
1983–84	U.S.	USSR	Syria	S

Source: For a list of sources used to identify these cases see Huth, Bennett, and Gelpi 1992.
[a] Austria–Hungary.
[b] S = deterrence success; F = deterrence failure.

Notes

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1. Structural realism as a theoretical framework encompasses at least three major lines of argument: (1) the impact of the security dilemma, (2) the importance that states place on relative gains, and (3) the relationship between system uncertainty and the likelihood of war. We restrict our attention to the third of these but use the general label of structural realism for ease of presentation.

2. Waltz argues that a tripolar system is inherently unstable because two powers will align with one another and eliminate the third power (1979, 163).

3. Our use of the term *uncertainty* is different from uncertainty in decision theory (Luce and Raiffa 1957), where uncertainty refers to a situation in which the probability distribution over different outcomes of a choice is unknown. *System uncertainty* here refers to the riskiness of escalating a conflict independent of a decision maker's estimated probability of prevailing in the dispute.

4. In order to avoid repetition and awkward phrasing, we will refer hereafter to "system uncertainty" without directly linking it to the perceptions of decision makers.

5. In our statistical analysis, we control for the effects of the expected value of each decision alternative, which enables us to estimate the independent effects of risk propensity interacted with systemic uncertainty.

6. Risk propensity also affects how individuals choose between options that have *different* expected values in a similar manner. For greater detail, see Huth, Bennett, and Gelpi 1992.

7. We assume that value of the status quo is certain because the psychological conception of risk propensity we have presented requires that decision makers know with certainty whether they are in the domain of gains or losses.

8. In applying the structural realist model, however, we supplement this rational-choice approach with insights from cognitive psychology concerning the sources of risk propensities.

9. Fearon (1990) argues that these selection effects should lead us to expect that the signs on the coefficients will be *reversed*. However, following Achen (1986), we believe that the only general conclusion that can be reached is that positive (negative) coefficients will be driven downward (upward). As a result, it is possible but not necessary that signs will be reversed.

10. We draw on Bueno de Mesquita (1978), who identifies but does not test the links between systemic uncertainty, risk propensity, and conflict behavior.

11. By interacting risk propensity with system uncertainty, we reconcile the contrasting predictions of Waltz (1979) and Deutsch and Singer (1964) in their long-standing debate regarding the relative merits of bipolar and multipolar systems. In order to be logically consistent, Waltz must implicitly assume risk-acceptant states in his work, whereas Deutsch and Singer must assume the opposite (Bueno de Mesquita 1978). Thus Waltz's contention that multipolarity increases the likelihood of conflict is logically correct only with regard to risk-acceptant decision makers. Similarly, Deutsch and Singer's argument that multipolarity is less conflict-prone is sustainable only with regard to risk-averse decision makers.

12. The list of authors who have discussed the importance of the balance of military forces is much too long to cite fully here, but a few examples are Bueno de Mesquita 1981, Huth 1988, Mearsheimer 1983, Powell 1990, and Wu 1990.

13. Consistent with the logic of our hypotheses concerning the defender's possession of a second-strike capability, we would test the following hypotheses regarding the challenger's possession of nuclear weapons: 1) In a situation of mutual assured destruction, the challenger's nuclear capability is of no coercive value because such coercive threats would lack sufficient credibility in the face of a retaliatory threat. Hence a challenger would not be more likely to escalate a dispute in such a situation. 2) In the absence of a situation of mutual assured destruction, the challenger's nuclear forces would be of coercive value, and thus the challenger would be more likely to escalate. Unfortunately, our population of cases does not allow us to test these hypotheses along with hypothesis 8. High levels of colinearity between the defender's possession of a second-strike capability and a situation of mutual assured destruction prevents us from obtaining reliable separate coefficients for these variables. In addition, our data set contains only three cases of the challenger's possessing nuclear weapons while the defender does not (see Appendix).

14. The hypothesis that decision makers will become more resolved following a capitulation in order to recoup reputational losses cannot be incorporated into a rational framework in which preferences are fixed because decision makers were aware of the reputational costs when they chose to capitulate.

15. We required that a deterrence encounter had to last at least one week with a militarized response by both sides in order to minimize the chance of including minor border incidents as well as cases in which great powers did not even consider escalation because the issues at stake were of limited political or strategic importance (e.g., fishing-boat incidents and airspace violations).

16. The cases listed in the Appendix draw in part on our previous work (Huth, Bennett, and Gelpi 1992). However, this data set includes a broader population of disputes since we no longer restrict our attention to great-power rival dyads.

17. The reason we do not simply code the dependent variable based upon the occurrence of war is that the absence of war does not necessarily indicate deterrence success. In some cases, the defender will capitulate precisely because it believes that it will be unable to deter the challenger.

18. For contrasting views regarding both the identification of deterrence encounters and the coding of deterrence success, see Lebow and Stein 1990. For a detailed discussion of the logic underlying the coding procedure which we use, see Huth and Russett 1990.

19. The demographic and industrial capabilities of states are not included in our measure because previous work finds that decisions to go to war are based generally on calculations of whether victory can be attained in a quick armed conflict (e.g., Anderson and McKeown 1987; Huth 1988; Mearsheimer 1983).

20. The bivariate correlations among components 2–6 are

	2	3	4	5
1. No. of great powers	.76	.12	−.28	.75
2. No. of clusters	—	−.07	−.41	.86
3. Diffusion among great powers	—	—	.51	.14
4. Diffusion among clusters	—	—	—	−.22
5. Cross-cutting ties	—	—	—	—

Factor analysis was performed using oblimin rotation in order to allow for correlation between the factors. Correlation between factors is .04. The factor loadings on factors 1 and 2, respectively, are *number of great powers* .806, .004; *number of clusters* .867, −.036; *average alliance tightness across clusters* .842, −.127; *diffusion of power across great powers* .284, .751; and *diffusion of power across clusters* −.102, .773. For a discussion of factor analysis techniques, see Kim and Mueller 1978. Henceforth, factors 1 and 2 will be referred to as system uncertainty 1 (system size) and system uncertainty 2 (capability diffusion), respectively.

21. We recognize that Bueno de Mesquita (1985) has refined this measure. However, his new risk propensity indicator is continuous, and its use here would make the interpretation of our results concerning the interaction of risk and system uncertainty quite difficult.

22. This index includes iron and steel production and energy consumption for all years. Oil production is added in 1900, and aluminum production is added in 1930.

23. We code this variable as a dummy instead of using the actual number of disputes because we almost never observe involvement in more than one other dispute in our data set. Consequently, we are unable to draw reliable conclusions regarding the effects of additional disputes.

24. One equation we were unable to estimate because of multicolinearity included the number of alliance clusters, the distribution of capabilities among clusters, and the level of ties across cluster boundaries.

25. Auxiliary regressions show in each specification that multicolinearity among the independent variables is not a problem.

26. Some readers may be concerned that our estimates concerning the effects of system uncertainty in interaction with risk propensity might be damaged by the fact that our measure of risk acceptance may be correlated with preventive or diversionary incentives for conflict. However, additional probit analyses in which we controlled for the independent effects of these factors indicated that their inclusion in the analysis did not change our estimates of the other variables.

27. Additionally, we believe that selection effects may explain the differences between our results and those of Leng (1983). He finds that in repeated confrontations, states that have lost a previous encounter with their adversary tend to resort to more coercive bargaining strategies in the following dispute. Leng's study selected only cases of at least three repeated confrontations between the same adversaries. As a

result, Leng excludes cases in which a challenger, after having been defeated once or twice, fails to initiate a third dispute. It seems likely that Leng's analysis focused on a set of particularly motivated challengers.

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NORMATIVE AND STRUCTURAL CAUSES OF DEMOCRATIC PEACE, 1946–1986

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Democratic states are in general about as conflict- and war-prone as nondemocracies, but democracies have rarely clashed with one another in violent conflict. We first show that democracy, as well as other factors, accounts for the relative lack of conflict. Then we examine two explanatory models. The normative model suggests that democracies do not fight each other because norms of compromise and cooperation prevent their conflicts of interest from escalating into violent clashes. The structural model asserts that complex political mobilization processes impose institutional constraints on the leaders of two democracies confronting each other to make violent conflict unfeasible. Using different data sets of international conflict and a multiplicity of indicators, we find that (1) democracy, in and of itself, has a consistent and robust negative effect on the likelihood of conflict or escalation in a dyad; (2) both the normative and structural models are supported by the data; and (3) support for the normative model is more robust and consistent.

Recognition of the *democratic-peace* result is probably one of the most significant nontrivial products of the scientific study of world politics. It may also be the basis of far more important insights into the workings of the international political world in modern times (Levy 1988; Ray 1992, chap. 6; Russett 1990, chap. 5). This result consists of two parts of equal importance: (1) democratic states are in general about as conflict- and war-prone as nondemocracies; and (2) over the last two centuries, democracies have rarely clashed with one another in violent or potentially violent conflict and (by some reasonable criteria) have virtually never fought one another in a full-scale international war.

Beyond the extraordinary convergence of research results that confirm that "democracies rarely fight each other" (see Maoz and Abdolali 1989 and Russett 1993 for reviews), there is, more importantly, significant evidence that this finding is causally meaningful. There is something in the internal makeup of democratic states that prevents them from fighting one another *despite the fact that they are not less conflict-prone than nondemocracies*. Attempts to attribute this result to factors other than the democratic system of the states revealed that the relationship between democracy and peace is probably not a spurious one (Bremer 1992; Maoz and Russett 1992). Disputes between democracies are far less likely than expected when compared with disputes between rich, rapidly growing, noncontiguous, and allied states.

The robustness of this result and its theoretical and practical significance call for a deeper inquiry into the causes of democratic peace. Specifically, a better understanding is required of the causal mechanism explaining simultaneously both the democratic-peace phenomenon and the lack of difference between democracies and nondemocracies in terms of their overall conflict proneness. This study continues and extends a number of inquiries on democratic peace by addressing the following questions: (1) Does the degree of democratization of a dyad, in addition to the effort of other factors, reduce its likelihood to engage in conflict? (2) What specific factors in the politics and norms of democratic societies prevent them from fighting one another? (3) Why is it that the same factors that prevent democracies from fighting one another fail to reduce the general rate of conflict involvement of democratic states?

We shall outline two principal explanations that have been invoked to account for the democratic-peace phenomenon, derive the logical and empirical implications of each of these explanations, and test the deduced propositions on the contemporary international system.

THEORETICAL CONSIDERATIONS

The empirical findings on the democratic-peace proposition present us with a seeming paradox, because there appears to be a contradiction between its two parts. This requires that any explanation of the democratic-peace phenomenon must simultaneously account for two observations that connect democratic political systems to international conflict. Any explanation that accounts for only one observation is incomplete and hence cannot be acceptable theoretically.

We will examine herein only two of the many possible explanations of these two observations. We regard these as the most general and potentially powerful explanations of the democratic-peace result. We label these two explanations as the normative and structural models of democratic peace.

The Normative Model

Elements of this model can be traced back to political thinkers such as Immanuel Kant and Woodrow Wilson; it is also represented by such modern scholars as Doyle (1986). It is based on two basic assumptions.

NORMATIVE ASSUMPTION 1. *States, to the extent possible, externalize the norms of behavior that are developed within and characterize their domestic political processes and institutions.*

NORMATIVE ASSUMPTION 2. *The anarchic nature of international politics implies that a clash between democratic and nondemocratic norms is dominated by the latter, rather than by the former.*

Assumption 1 suggests that different norms of domestic political conduct will be expressed in terms of different patterns of international behavior. Democratic regimes are based on political norms that emphasize regulated political competition through peaceful means. Winning does not require elimination of the opponent, and losing does not prohibit the loser from trying again. Political conflicts in democracies are resolved through compromise rather than through elimination of opponents. This norm allows for an atmosphere of "live and let live" that results in a fundamental sense of stability at the personal, communal, and national level. We term these *democratic norms.*

In contrast, political competition in nondemocratic regimes is likely to be more zero-sum in terms of the conception of the parties and in its consequences. The winner may take all, denying the loser the power or opportunity to rise again. Political conflicts in nondemocratic regimes are more likely to be conducted and resolved through violence and coercion. This norm creates an atmosphere of mistrust and fear within and outside the government. Stability may be maintained only in the absence of an overt and effective political opposition. This is the essence of *nondemocratic norms.*

Assumption 2 deals with the limits of the ability to apply certain norms in an anarchic international system. In such a system, states put their survival above any other value they seek to promote. If states come to believe that their application of domestically developed democratic norms would endanger their survival, they will act in accordance with the norms established by their rival. Democratic norms could be more easily exploited than could nondemocratic ones. Hence democracies are more likely to shift norms when confronted by a nondemocratic rival than is the nondemocratic rival to shift to democratic norms of international conduct.[1]

It follows that when two democracies confront one another in conflicts of interest, they are able effectively to apply democratic norms in their interaction, thereby preventing most conflicts from escalating to a militarized level, involving the threat, display, or use of military force, and—of course—from going to all-out war. However, when a democratic state confronts a nondemocratic one, it may be forced to adapt to the norms of international conduct of the latter lest it be exploited or eliminated by the nondemocratic state that takes advantage of the inherent moderation of democracies.

A conflict between nondemocracies would be dominated by the norm of forceful conduct and by both parties' efforts to resolve the conflict through a decisive outcome and elimination of the opponent. Thus, conflicts between nondemocracies are more likely to escalate into war than are conflicts between a democratic and nondemocratic state.

In disputes between democracies, however, the expectation that conflicts can be settled peacefully, by compromise, lowers the relative benefit to be achieved from violence. Dependence on democratic norms tips rational cost–benefit calculations toward further support of those norms. Empirically, disputes between democracies are more likely to be settled by third-party conflict management, by agreement or stalemate (rather than an imposed solution), and by strategies of reciprocation (Dixon 1993; Leng 1993).

Political culture and political norms constitute images that a state transmits to its external environment. One of the most important images that a democratic state can communicate to its environment is a sense of political stability. Likewise, instability conveys images linked with nondemocratic states. We elsewhere specify just why instability or the perception of instability may work to encourage the use of force by an unstable regime or to identify an unstable regime as the object for the exercise of the use of force (Huth and Russett 1993; Maoz 1989; Maoz and Russett 1992).

Perceptions of instability may be based on the recency and immaturity of experience with democratic processes and norms; a new democracy will not yet have developed wide experience in practices of democratic conflict resolution. Perceptions of instability may also be based on a high degree of violent opposition to the democratic government; a democracy under seige of domestic terrorism, insurgency, or civil war is one in which the ostensible norms of peaceful conflict resolution simply are not working well. To the degree that the practice of democratic forms of government is very recent, subject to violent domestic challenge, or incomplete, it may be imperfectly constrained by the norms of democratic government that are supposed to keep conflict nonviolent. Or uncertainty about the commitment to democratic norms by a state with which one has a conflict of interest may lead to perceptions and expectations that it will practice those norms imperfectly.

The Structural Model

This model was discussed by modern students of international conflict (e.g., Bueno de Mesquita and Lalman 1992; Rummel 1979, vol. 4; Rummel 1983; and Small and Singer 1976).[2] It rests upon the following assumptions:

STRUCTURAL ASSUMPTION 1. *International challenges require political leaders to mobilize domestic support to their policies. Such support must be mobilized from those groups that provide the leadership the kind of legitimacy that is required for international action.*

STRUCTURAL ASSUMPTION 2. *Shortcuts to political mobilization of relevant political support can be accomplished only in situations that can be appropriately described as emergencies.*

International action in a democratic political system requires the mobilization of both general public opinion and of a variety of institutions that make up the system of government, such as the legislature, the political bureaucracies, and key interest groups. This implies that very few goals could be presented to justify fighting wars in democracies. It also implies that the process of national mobilization for war in democracies is both difficult and cumbersome. On the other hand, in nondemocratic societies, once the support of the key legitimizing groups is secured, the government can launch its policy with little regard to public opinion or for due political process. Because, in many cases, the legitimizing groups may benefit from the use of force in foreign affairs, the leadership may feel little restraint in its dealings with other states.

This set of assumptions implies, therefore, that due to the complexity of the democratic process and the requirement of securing a broad base of support for risky policies, democratic leaders are reluctant to wage wars, except in cases wherein war seems a necessity or when the war aims are seen as justifying the mobilization costs. The time required for a democratic state to prepare for war is far longer than for nondemocracies. Thus, in a conflict between democracies, by the time the two states are militarily ready for war, diplomats have the opportunity to find a nonmilitary solution to the conflict.

Conflicts between a democracy and a nondemocracy, however, are driven by the lack of structural constraints on the mobilization and escalation process of the latter. The democratic state finds itself in a no-choice situation. Leaders are forced to find ways to circumvent the due political process. Thus, in such a conflict, the nondemocracy imposes on the democratic political system emergency conditions enabling the government to rally support rather rapidly.

Conflicts between nondemocratic systems are, by the same token, likely to escalate because both leaderships operate under relatively few structural constraints. The failure of initial efforts to find a peaceful solution may result in a rapid flare-up of the conflict into a violent level.

Comparing the Models

These two explanations are not mutually exclusive. They do emphasize, however, two different facets of democratic politics that are presumably responsible for the democratic-peace phenomenon. The structural model views the constitutional and legal constraints on executive action as a key to understanding how governments act in their international politics. The normative model looks primarily at the effects of norms of domestic political behavior on international politics.

Obviously, it is extremely difficult to distinguish between these models in terms of contradictory predictions. Normative and structural explanations are often not well differentiated conceptually, thus enhancing the difficulties of testing them as alternative hypotheses.[3] For example, both models would claim that the tendency toward conflict decreases with the extent of political participation in a society. The normative model explains this relationship in terms of a correlation between political participation and democratic norms. The structural model explains this relationship in terms of a correlation between political participation and structural constraints on the executive's ability to use force. There may be, however, a number of areas where the models differ in their predictions. Two, in particular, come to mind. First, democratic norms take time to develop. Hence if the normative model is right, then older democracies should be less likely to clash with one another than would newer ones. The structural model would claim that as long as structural constraints operate on the executive, the age of the political regime should not matter. Second, the structural model implies variations between democracies in terms of their conflict behavior. Presidential systems should be less constrained than parliamentary systems, in which the government is far more dependent on the support it gets from the legislature. Coalition governments or minority cabinets are far more constrained than are governments controlled by a single party. On the other hand, the normative model does not expect variation within democratic political systems; despite different structures, they operate within the same normative system.

Before examining the two models, however, it is important to assess the extent to which democracy, relative to other variables, accounts for the conflict involvement patterns of international dyads. For the purpose of such an analysis, we reiterate briefly the factors that have been variously mentioned as potential causes of democratic peace, outside of the realm of democracy.

OTHER POTENTIAL CAUSES

Three other potential causes of democratic peace should be considered.[4] First, rich states do not fight one another because they have far more to lose than to gain by doing so. Rich states are often engaged in heavy trading with one another. The costs of a war would be enormous and the benefits would be little. Since most democracies in the post–World War II era were economically developed states, it was their economic structure, rather than their type of political system, that prevented them from fighting one another.

Second, rapidly growing states would harm themselves by engaging in conflict against other rapidly growing states—again, because conflict and war would harm the economic benefits associated with growth. Fighting other rapidly growing states is both more costly and risks reversing the positive economic-growth pattern. Most democracies experienced rapid economic growth and for that reason refrained from conflict with each other.

Third, most democracies in the post–World War II era have been in some sort of a direct or indirect alliance with one another.[5] These alliance bonds, rather than their political system, prevented them from fighting one another.

In addition to these factors, we examine the potentially confounding effects of geographic contiguity and military capability ratios on dyadic conflict involvement. These factors are included because they have been found to be highly potent predictors of conflict escalation (Bremer 1992; Geller and Jones 1991; and Siverson 1991).

RESEARCH DESIGN

The normative–cultural and the structural–institutional models suggest several testable hypotheses. In addition, multivariate statistical analysis allows assessment of how far each of various influences other than type of political system (e.g., contiguity, wealth, economic growth, alliance, and military capability ratio) affects conflict. Critical tests allow for a competitive and simultaneous assessment of the relative power of the two models. We test these hypotheses:

HYPOTHESIS 1. *The more democratic are both members of a pair of states, the less likely it is that militarized disputes break out between them, and the less likely it is that any disputes that do break out will escalate. This effect will operate independently of other dyadic attributes (e.g., wealth, economic growth, contiguity, alliance, capability ratio).*

HYPOTHESIS 2 (NORMATIVE MODEL). *The more deeply rooted are democratic norms in the political processes operating in two states, the lower the likelihood that disputes will break out or that disputes will escalate.*

HYPOTHESIS 3 (STRUCTURAL MODEL) *The higher the political constraints on the executives of the two states, the lower the likelihood that disputes will break out or that disputes will escalate.*

Spatial–Temporal Domain

We look at pairs of independent states in the world during the period 1946–86, in essence, at the Cold War era. This era is appropriate for three reasons. First, although a score or more of democracies existed in the first half of the twentieth century, the number of pairs of democratic states was three times as large in the later era.

Second, as a "nice" generalization at least partly context-dependent, the role of democracy in restraining violent conflict between democratic dyads may have been stronger in the past half-century than earlier (Most and Starr 1989). Democratic norms have become deeply entrenched, since many states have been democracies for long periods and principles such as true universal suffrage have been put into practice. Similarly, many countries' democratic institutions have been reinforced over time. Continuity of democracy in a state encourages its partners in foreign affairs to perceive it as stably democratic. The experience of three world "wars" (World War I, World War II, and the Cold War)—each characterized by both rhetoric and some reality as a conflict of democracies against authoritarian states—helped build normative principles that democracies ought not to fight among themselves.

Third, many influences put forward as confounding and contributing to the phenomenon of peace between democratic states were much more prominent after World War II. The post-1945 era brought unprecedented global wealth and growth, and the alliance system was far wider and more durable than any that preceded it. Thus a more complex test of the basic hypothesis becomes possible—a test designed to display the power of competing hypotheses. Moreover, data on economic levels and growth rates are much more reliable and widespread for the past half-century than before.

Our unit of analysis is the dyad-year; we look at each pair of countries in each year to see whether they engaged in any kind of militarized dispute. Over the period 1946–86 the international system averaged about 110 countries per year, which would give us roughly 265,000 dyad-years to study. But the vast majority are nearly irrelevant. The countries comprising them were too far apart and too weak militarily, with few serious interests potentially in conflict, for them plausibly to engage in any militarized diplomatic dispute. Contiguity and major-power involvement are the two most important static factors accounting for the likelihood of war between any pair of states (Bremer 1992). If we limit the analysis to pairs of states that are directly or indirectly contiguous or in which one member is a major power (*contiguity* and *major power* will be defined), we have a total of 36,162 dyad-years, with disputes occurring in 714 of them by the Militarized Interstate Dispute (MID) data set or 448 by the International Crisis Behavior (ICB) data set.[6]

Some disputes do, of course, arise between "implausible" pairs, as between a minor European power like Belgium or the Netherlands and a former colony or the case of distant collective security action, as in Korea and Vietnam. In dropping all but about 12% of total dyad-years, the list of plausible pairs nevertheless retains 74% of disputes in the MID data set and 80% in the ICB one. In the more comprehensive MID data it picks up 78% of all the disputes that democracies engaged in with anyone and all but one of the 15 disputes between democracies. Thus we are fairly confident that no major case-selection biases exist in favor of the hypothesis, and the refined "universe" of politically relevant dyads is theoretically appropriate.

Data and Measurement

We want to explain patterns of conflict. The conflict data are from two different data sets, compiled for somewhat different analytical purposes and using different definitions. That allows us to establish whether our conclusions remain consistent over different measures of the concepts. The more robust the results are to such changes (in measures of independent variables, as well as conflict), the more confidence we can have in the generalization.

Dependent Variables. One data set is the MID data from the Correlates of War (COW) project. These data were derived from a set developed for the period 1816–1976 (Gochman and Maoz 1984). They were updated to 1986 by Maoz and compared (with a nearly perfect match) to a list produced by Daniel M. Jones of the University of Michigan. A MID is defined as "a set of interactions between or among states involving threats to use military force, displays of military force, or actual uses of force. To be included, these acts must be explicit, overt, nonaccidental, and government sanctioned" (Gochman and Maoz 1984, 586). The MID data lists the starting and ending date for each dispute and the states that participated on each side. A dispute with three states on one side and four on the other makes 12 dispute dyads. We use the data in two forms. First, we identify each dyad-year dichotomously as having some kind of dispute or none. In doing so, we include both disputes begun any time in this year and ongoing disputes that continued into this year from a previous one. This variable is labeled *dispute involvement*. Second, we record the highest level of hostility reached by either member of the dyad in that year, using the Gochman–Maoz five-level scale of hostility.[7] This is termed *dispute escalation*.

The other set of conflict data is that collected by the ICB project (Brecher and Wilkenfeld 1989; Brecher, Wilkenfeld, and Moser 1988). Its compilers define an international crisis as "a situational change characterized by an increase in the intensity of disruptive interaction between two or more adversaries, with a high probability of military hostilities. . . . The higher-than-normal conflictual interactions destabilize the existing relationships of the adversaries and pose a challenge to the existing structure of an international system—global, dominant, and/or subsystem" (Brecher and Wilkenfeld 1989, p 5). Levels of hostility for international crises are the same as for disputes.

The two data sets are not strongly related. Due to different definitions and criteria, among politically relevant dyads there are 959 with MID conflicts begun or underway, only 260 (27%) of which were identified by the ICB data set. This is not surprising, given the latter's concern with "a high probability of military hostilities" and the likelihood that many MIDs neither carried (nor, often as symbolic acts in a bargaining process, were they always intended to carry) great likelihood of escalating to actual violence. It is also true, however, that out of the 359 politically relevant crisis dyads identified by ICB listing, only 260 (72%) are found in the MID data. This is not to imply that either set is inaccurate; rather, there is sufficient variability in case identification to enable us to use the two data sets as a check on the robustness of our results.

Independent Variables: Democracy. Our foremost independent variable is of course form of government, or "regime." Our chief source of data here is developed from the Polity II data (Gurr, Jaggers, and Moore 1989). It updates and extends data collected earlier (Gurr 1974) based on the regime classification of Eckstein and Gurr (1975). We defined the type of regime as follows. First, we identified the level of authority of a political system as a combination of (1) competitiveness of political participation, (2) regulation of participation, (3) competitiveness of executive recruitment, (4) openness of executive recruitment, and (5) constraints on the chief executive, following Gurr, Jaggers, and Moore 1989. Their aggregation of these dimensions produced one 11-point scale for the level of democracy (DEM) and another for autocracy (AUT) (pp. 36–39).

Second, because the Eckstein–Gurr conception is not linear, a state can have mixed characteristics; some features may be democratic at the same time that others are highly autocratic. Indeed, in the Polity II data set the correlations are negative and high, but far from perfect.[8] Therefore, we created a *continuous* index taking into account both democratic and autocratic features—and also the level of power concentration, which reflects how far the state authorities exercised effective control over their constituents. This measure of power concentration (PCON) is also an 11-point scale from 0 to 10 (Gurr, Jaggers, and Moore 1989, 39–40). The regime index (REG) then is defined as REG = PCON(DEM − AUT), with a possible range from −100 (most authoritarian) to +100 (most democratic). Toward the extremes, these judgments are not problematic, but around zero the regime characteristics are not clearly defined. Either democratic and authoritarian features may cancel each other out if a state scores fairly high on both, or the power concentration score may be so low that even if the regime is predominantly democratic or authoritarian the characteristics cannot effectively express themselves. This situation is common in highly unstable political systems or in systems undergoing rapid change. Though more recent than these codings, conditions in the Soviet Union in 1990–92 offer a good example.

Then, we needed to convert the individual scores into a joint democratization one because our analysis requires a dyadic characterization of regime type. The joint measure (JOINREG) must reflect two things simultaneously, namely, How democratic or undemocratic are the members of the dyad? and How different or similar in their regime types are the two states? Our measure is

$$\text{JOINREG} = \frac{\text{REG}_h + \text{REG}_l}{\text{REG}_h - \text{REG}_l + 1},$$

where REG_h is the regime score of the member with the higher score and REG_l that of the lower-scoring member.[9]

We also needed an alternative measure, transforming the continuous regime score into a *discrete* (dichotomous) regime type. Some common hypotheses say that the more democratic both members of the pair, the less likely they will become embroiled in a militarized dispute, but others simply posit a difference in conflict behavior between different regime categories. Moreover, our continuous index is generated by an arithmetic operation performed on ordinal variables. Since the ordinal variables (DEM, AUT, PCON) are probably not linear within categories, the overall index may be only crudely reliable—across certain ranges but not for specific values (e.g., between states scoring 35 and 50).

We use a threshold of +30 as the lower limit for democracies and categorize all states with scores from −25 onward as authoritarian. (States scoring between these two points, with a mixture of democratic and authoritarian characteristics or a low concentration of power, are termed anocratic; see Gurr 1974; Maoz and Abdolali 1989.) With the simple categorization of each regime as democratic or not (combining autocratic and anocratic), we have a dichotomous variable of democratic–democratic pairs and all others, consistent with the hypothesis that democratic pairs are different from all other kinds of pairs. Virtually all previous empirical studies investigating the relationship between democracy and international conflict have employed some threshold for establishing categories of regime types (e.g., Bremer 1992; Chan 1984; Doyle 1986; Maoz and Abdolali 1989; Morgan and Campbell 1991; Morgan and Schwebach 1992; Rummel 1983; Small and Singer 1976; Weede 1984). We innovate in using the continuous version.

Whether in continuous form or as dichotomized, we prefer our multidimensional regime index to the Gurr–Jagger–Moore 11-point index employed in other studies. In the 1946–86 era, 338 nation-years (nearly 22% of all democratic nation-years) would have been characterized as democratic on the Gurr index of democracy alone (ignoring his autocracy scale) but not on our multidimensional index. Some major cases lack face validity. For example, Gurr's democracy score for Rhodesia was 7 for 1965–78, as was South Africa's for the entire period; on our scale, both received 16, well below the democracy threshold of 30. India had a Gurr score of 9 during the 1975–79 period of emergency rule limiting fundamental democratic rights, whereas it scored only 27 (slightly below the threshold) on our index.

We created an alternative measure from data of Arthur Banks (1986) included in the Polity II data set. We identified democratic states as those in which both legislature and executive were selected in a competitive election and in which the legislature was at least partially effective. This simpler categorization is less fully documented than Gurr's. The two are moderately correlated, suggesting, as with the two conflict data sets, that each measures a similar concept but with enough difference to provide a good test for robustness.[10]

Degree of Institutional Constraints. To distinguish between the two models for explaining the rarity of conflict between democracies, we used several key attributes identified by Gurr and his associates (Eckstein and Gurr 1975; Gurr, Jaggers, and Moore 1989). We constructed a multifaceted measure from related but distinguishable elements, in which an executive is considered to be subject to the least restraint when able to operate by "one-man rule," without institutionalized constraint, in a centralized political system in which the government exerts a wide scope of control over economic and social life.

Degree of "one-man rule" (*monocratism*) ranges on a five-point ordinal scale from states where it prevails to "those in which some kind of assent is required, whether by especially prestigious minorities . . . numerical majorities, or virtually all of them" (Eckstein and Gurr 1975, 375). *Degree of executive constraint* represents the extent to which the executive must abide by clear and distinguishable rules—institutionalized constraints—while making policy decisions, whether the chief executive be an individual or collectivity, measured on a seven-point ordinal scale. *Centralization* distinguishes between unitary and federal political systems, on a three-point ordinal scale. As Gurr, Jaggers, and Moore point out: "Federal polities have greater complexity of Conformation than do centralized polities. Opportunities for participation also tend to be higher in federal systems, and regional units of government potentially are more responsive to local inputs than are centralized governments" (1989, 21). Federalism is probably not as severe a constraint on foreign policy as on domestic policy, but even on foreign policy it somewhat restricts the ability to mobilize economic and political resources rapidly in the event of a serious international dispute. It also provides an institutionalized base from which regional political leaders can challenge government policy. *Scope of government actions* "refers to the extent to which all levels of government combined—national, regional, and local—attempt to regulate and organize the economic and social life of the citizens and subjects." It is measured on a seven-point scale from *totalitarian*, or those governments that "directly organize and control almost all aspects of social and political life," to *minimal*, or those whose operations are largely or wholely limited to such core functions as maintenance of internal security and administration of justice" (pp. 21–22).

These four measures are summed over their categories to produce an overall scale of institutional constraints ranging from 4 (a totalitarian system lacking any form of constraint) to 22 (a highly constrained political system in which the government must go through a long, complex, and uncertain political process to invoke national action). As with regime type, we divided the scale into three levels (4–10, 11–15, and 16–25) and for a dichotomized variable defined *high constraint* as 16 and above. This measure is related to, but makes substantial differentiations from, the measure of democracy, suggesting that we can validly use it as an independent measure to test the structural explanation.[11] Democracies exhibiting low constraint include the French Fifth Republic under Charles DeGaulle and Georges Pompidou, Venezuela after the 1958 overthrow of the military dictatorship, and Argentina under the elected government of the Perons in 1973–75. Nondemocratic governments operating under rather high constraint include Pakistan shortly after independence, Indonesia into 1956, and several Middle Eastern states in the 1950s (King Hussein's Jordan being the clearest example).

Democratic Norms. The extent to which some norms of democratic behavior have become accepted in a political regime may not be closely related to states' political structures. For example, a system may lack a democratic institutional structure yet be widely regarded by its citizens as politically legitimate; such a regime would require little overt oppression of opposition in ways obviously violating democratic norms. On the other hand, a democratic government undergoing violent insurgency and a fundamental crisis of legitimacy may resort to political and military oppression in the name of maintaining public order and, indeed, of maintaining democratic institutions.

We employ two related but distinct ways of measuring the extent to which democratic or other kind of norms operate in a society. The first is through the concept of political stability. It is based on the notion that it takes time for norms to develop. A society that undergoes fundamental change requires a considerable period of time to develop norms of political conduct and for the citizens to internalize those norms and become accustomed to them. The longer a given political system or regime exists in a society without fundamental change, the more likely that norms of political conduct, whether democratic or nondemocratic, will form and influence the foreign policy codes of conduct of the regime.

We can then measure the prevalence of political norms in a society as the persistence of its political regime in years (Gurr 1974). By this conception, democracies that are highly stable (i.e., have kept their fundamental political structure for a long time) are said to be more influenced by democratic norms than democracies that have existed only a short while. Conflicts between stable democracies should thus be far less common than conflicts between democracies in which one (or, worse, both) are unstable. Note that our stability measure is not fully distinct from structures. It can also be an institutional constraint in the limited sense than an unstable democracy is subject to overthrow, releasing the institutional constraints on leaders. Also, we are measuring the duration of political institutions more directly than the norms that support them. Nonetheless, this measure still seems separable from the indices we introduced to measure the strength and breadth of institutions.

An alternative procedure for measuring democratic norms relies directly on the level of violent internal social and political conflict. All states experience some degree of social conflict. The difference between states where democratic norms prevail and states where they do not, however, is twofold. First, in democracies these conflicts are predominantly nonviolent; both challengers and defenders of the status quo usually find peaceful avenues for expressing their differences. In nondemocratic systems conflicts are likely to take on violent forms because most forms of peaceful protest are forbidden. Second, in a democracy, the government rarely needs to use force to resolve conflicts; order can be maintained without violent suppression. But in nondemocracies, order is often maintained by overt state violence. Democratic norms are tested in times of political unrest and instability.

Therefore, we measure democratic norms by the amount of political violence within a state. Two types of measures are used: deaths from political violence and extent of domestic conflict. First, from data reported by Taylor and Jodice (1983), we use two related indicators: the number of deaths from political violence indicates the general level of domestic violence in a state, and the number of political executions indicates the degree of regime-initiated violence. The definition of democratic norms is the average number of deaths from domestic political violence (or the average number of political executions) over the last five years per state, averaged over the dyad. Specifically,

$$\text{Pol Deaths} = \frac{1}{2}\sum_{j=1}^{2}\frac{\sum_{t=0}^{-4}\text{POLDTHS}_{jt}}{5},$$

where t is a given year and j is an index of the member of the dyad.[12] For a dichotomized variable, the scale is divided at the mean.

Second, the COPDAB domestic data set (Azar 1980) contains information about both conflictual and cooperative political events within states. These events are placed on a 14-point scale. Scores 1–7 represent high-to-low cooperation, a score of 8 represents neutral actions, and scores 9–14 represent low-to-high conflict. Since the unit of analysis in the COPDAB data set is an event, we first had to aggregate the scale values for each of the conflictual and the cooperative events separately over the each year.[13] The measure of conflictual events was similar to that of political deaths. Specifically,

$$\text{Conf Event} = \frac{1}{2}\sum_{i=1}^{2}\frac{\sum_{t=-5}^{0}(\text{SumConf}_{it} - \text{SumCoop}_{it})}{5},$$

where SumConf and SumCoop are, in a given year, the weighted sums of conflictual and cooperative events, respectively. Here again, the joint conflict event measure is an average, over both states, of the mean level of net conflict in each state over the last five years.

Wealth. Average levels of income were rising over the period, so we needed a measure of relative rather than absolute wealth. Since the standard economic data are delineated in U.S. dollars, we simply used the cross-national estimates (Summers and Heston 1988) as a baseline for each year. The income data produced a continuous dyadic measure computed in the same way as that for regimes (JOINREG).

Economic Growth. Economic growth is the percentage change in a state's gross domestic product (in constant 1980 prices) from one year to the next, computed as the average growth rate over the three years preceding the first year.[14]

Alliance. Alliance data have been compiled as part of the COW project (Singer and Small 1968), to which we added a category for indirect alliance with the United States. An indirect alliance occurs where two states which have no direct alliance with each other are each allied individually with another. Weede (1983) reasons that restraints imposed by the "hegemon" may moderate disputes between indirectly linked states. We use a dichotomous break between any direct or indirect alliance and none.[15]

Contiguity. Here too we used a revised version of a COW data set listing several degrees of contiguity, to which we added colonial contiguity for cases where one state bordered another's colony or trusteeship.[16] Conceptually, contiguity is meant to identify states with the capability and possible reason for fighting each other, so our sample also includes all dyads containing a major power with the ability to exert military force beyond immediately contiguous states. We identified the United States, United Kingdom, France, and the Soviet Union as major powers and (perhaps more arguably) followed the COW designation of China as a major power from 1950 onward. This procedure is close to that used by Weede (1983) to test for conflict only among "strategically interdependent" dyads. The major difference is our inclusion of France and Britain to pick up their many postcolonial conflicts. We make a dichotomous break between any kind of contiguity and the noncontiguous dyads including a great power.

Military Capability Ratio. Are two states with similar capabilities more likely to dispute with each other than are states whose economic and military capabilities are very disparate? This question, vigorously debated without clear resolution, may confound this analysis. Power disparity represents one final control variable. We use the widely employed COW military capability index (Singer, Bremer, and Stuckey 1972). In effect, that composite index weights about equally (two separate indices for each) military forces in being, economic strength, and demography, suggesting both capacity for winning a short war with existing military forces and long-term capacity for waging a war of attrition. It only imperfectly reflects the perception or reality of military power (Russett and Starr 1992, 145–46) but is adequate here as an interval measure of the ratio of the capability score of the stronger state to the weaker.

Data Analysis Methods

Data analysis was done in three steps. The first step was designed to perform a multivariate analysis of the various factors that may support the hypothesis that the democratic-peace phenomenon is spurious. This is a replication and extension of earlier analyses we conducted (Maoz and Russett 1992). The second step in the analysis was to examine jointly the structural and normative models of democratic peace.[17] If one of the models were supported consistently while the other were rejected consistently, no critical testing would be required. But since both models received some empirical support, we moved to a third analysis with a critical test.

Design-related Problems

Use of the dyad-year involves a statistical problem in that a particular dyad's conflict status is not independent from one year to the next. It is complicated by the fact that in this analysis we treat a continuing conflict as present in each year, not just when it began—as, of course, continuing peace is counted for each year. More generally, the nonindependence inflates the apparent sample for statistical testing, lowering the threshold for a relationship to be considered significant.

Sensitivity checks indicate that our treatment of continuing conflicts does not materially change the results. In any case, using the dyad-year is unavoidable, given that states' political systems and other variables typically change frequently during the 40-year period; aggregation of the differences into a single value for the period would be meaningless. Moreover, realist theory itself implies that events are inherently interdependent because the structure of the system, rather than preferences of decision makers, "dictates" decisions on conflict and war. Consequently, a decision by one state to engage in conflict with another alters the structural constraints on other states, and the other's set of feasible actions is changed.[18]

The research design is a pooled time-series analysis. Many of the diagnostics appropriate to such analysis using multiple regression are unavailable when the dependent variable is dichotomous or ordinal; the necessary computing power is lacking. There is no easy way to know whether and to what extent results are biased by heteroscedasticity and autocorrelation. The realist variables (notably contiguity and capability ratio) and wealth are quite stable over time, introducing autocorrelation. One of the indirect ways of estimating the degree of autocorrelation is also problematic because the skewed nature of the dependent variable (i.e., the very low ratio of conflict to nonconflict years for each dyad) also introduces autocorrelation.

In order to redress this problem at least partially, we conducted a set of tests. First, we sorted our data set by dyad by year. Then we computed a lagged dichotomous conflict variable. Next we ran all of our logistic regressions twice—once with the lagged conflict variable included and once without it. In most of the cases and as we had suspected, the lagged conflict variable had a significant positive effect on the likelihood of conflict (both in the MID data set and in the ICB data set). However, the sign, magnitude, and significance level of the parameter estimates of all other variables in the equations did not change significantly in the case with the lagged conflict, compared to the case without the lagged conflict. This led us to conclude that the autocorrelation problem, though valid, does not have a major biasing effect on the results.[19]

RESULTS

We start by examining the effect of several variables that potentially confound the relationship between democracy and peace on dyadic conflict involvement and conflict escalation, along with the democracy variable. This test of hypothesis 1 is shown in Table 1. Table 1 shows the effect of the independent variables, measured in continuous terms (with exception of alliance and contiguity), on the dependent variables. Tests with dichotomized measures of democracy give similar or stronger results and need not be shown. In the upper half, the dependent variable is defined as the presence or absence of a dispute (crisis) between a pair of states at a given year. In the lower half, the dependent variable is defined as a five-point ordinal scale with 0 representing no dispute crisis and 4 representing a full-scale war.[20] The multiple dependent variables and the different measurement scales of the independent variables serve as a way of assessing the stability of the results and their robustness. Analyses performed on the same dependent variables using the Banks measure of democracy yielded consistently similar results.

Hypothesis 1 is—with some exceptions—supported by the data. In the MID data both the continuous version of democracy and the dichotomous one (not shown in Table 1) have a significant effect on conflict involvement. In the TCB data the continuous version of democracy is not significant, but the dichotomous version (not shown) is consistently related to crisis involvement. Among the confounding variables, almost all are related to both the MID measures of conflict and the ICB measures. The results for dispute or crisis escalation are nearly identical to those obtained for conflict involvement. The level of democratization has a significant main effect on dispute escalation, and when dichotomized, on crisis escalation even when we control for potentially confounding variables. Democracies are less likely to escalate disputes against other democracies than are states that have other types of political systems.

TABLE 1

Effects of Joint Democracy and Potentially Confounding Factors on Conflict Involvement and Escalation

INDEPENDENT VARIABLE	MILITARIZED DISPUTES[a]	INTERNATIONAL CRISES[b]
	Effect on Conflict Involvement	
Democracy	−.004 (.002)**	−.002 (.003)
Wealth	−.022 (.008)**	−.040 (.016)*
Growth	−.107 (.021)**	−.133 (.032)**
Alliance	−.517 (.105)**	−.339 (.165)*
Contiguity	1.419 (.108)**	1.964 (.190)**
Capability ratio	−.007 (.001)**	−.002 (.001)**
	Effect on Conflict Escalation	
Democracy	−.004 (.002)*	−.001 (.003)
Wealth	−.022 (.008)**	−.040 (.016)*
Growth	−.111 (.021)**	−.139 (.031)**
Alliance	−.522 (.105)**	−.336 (.164)*
Contiguity	1.417 (.108)**	1.962 (.190)**
Capability ratio	−.007 (.001)**	−.002 (.001)*

Note: N = 19,020. Entries are unstandardized parameter estimates in logistic regression equations; standard errors are in parentheses. Gamma is a measure of the difference between the observed and expected values throughout the analysis, appropriate for a priori prediction of monotonic relationships (Hildebrand, Laing, and Rosenthal 1977).
[a]Gamma = .54.
[b]Gamma = .59.
*$p < .05$.
**$p < .01$.

Taken together, these findings corroborate our bivariate results (Maoz and Russett 1992). Not surprisingly, power relationships make a big difference. Great disparities in power sharply discourage the expression of diplomatic disputes in any militarized form. Contiguity also matters, with its power-related emphasis on capability, as well as on the possibility of incentive for dispute. But the other variables also make a significant difference in almost every instance. The multivariate analysis also corroborates Bremer's (1992) findings regarding alliance effects on dispute involvement and dispute escalation. It appears that while the bivariate relationship between alliance and conflict is positive (Maoz and Russett 1992), after controlling for other relevant variables, allied parties are less likely to fight each other than would be expected by chance alone.

All the theories competing with that about democracy find solid support. Nevertheless, a strong, independent, and fairly robust role for joint democracy remains evident. In the ICB data democracy in continuous form is not significant, but the dichotomous version (democracy/nondemocracy) is. A strong relationship is apparent in the MID data in both contin-

TABLE 2

Effects of Joint Democracy and Potentially Confounding Factors on Conflict Involvement

	NORMS MEASURED BY STABILITY		NORMS MEASURED BY EXECUTIONS	
INDEP. VAR.	MILITARIZED DISPUTES	INTERNATIONAL CRISES	MILITARIZED DISPUTES	INTERNATIONAL CRISES
Political stability	−.053 (.013)**	−.111 (.031)**	—	—
Political executions	—	—	.272 (.044)**	.162 (.065)**
Institutional constraints	−.021 (.004)**	−.026 (.007)**	−.016 (.004)**	−.031 (.007)**
Wealth	−.022 (.007)**	−.034 (.015)*	−.018 (.007)**	−.024 (.014)
Capability ratio	−.009 (.001)**	−.002 (.001)**	−.008 (.001)**	−.002 (.001)**
Alliance	−.483 (.108)**	−.237 (.174)	−.570 (.114)**	−.365 (.182)*
Contiguity	1.225 (.104)**	1.846 (.188)**	1.176 (.108)**	1.747 (.193)**
Gamma	.54	.61	.54	.58
Number of cases	18,762		17,317	

Note: Entries are unstandardized parameter estimates in logistic regression equations; standard errors are in parentheses.
$^{*}p < .05$.
$^{**}p < .01$.

uous and dichotomous form; the more democratic each member of the dyad, the less likely is conflict. The phenomenon of democratic peace is real, not spurious.

We tested a large number of interaction effects between sets of variables. In the interest of brevity, we do not report these terms in the tables. In most of the analyses, interaction effects were not statistically significant. This applies to interactions between pairs of potentially confounding variables, as well as to interactions between each one of the confounding variables and democracy.

Given these findings, we can meaningfully assess the extent to which each of the two models discussed withstands an empirical test. Table 2 shows the effects of democratic norms (defined once in terms of political stability and once in terms of political executions) and institutional constraints on conflict occurrence. Equations for escalation produced almost identical results and need not be shown. The table reports only a summary of the runs we conducted in this set of analyses. Other tests with similar results included the use of alternative indicators of democratic norms (e.g., deaths from political violence, or level of domestic conflict from COPDAB).[21] Both models seem to be supported by the data. When defined as continuous variables, both institutional constraints and democratic norms reduce national conflict involvement and conflict escalation. Here, too, the relationship is generally robust: it holds *across conflict data sets* and is invariant to definitions of the independent variables. The effects of both norms and institutional constraints on conflict involvement and conflict escalation hold fairly consistently *even when we control for the potentially confounding factors* that have been mentioned by other theories as nonregime causes of democratic peace. However, when institutional constraints and democratic norms are dichotomized (low and high constraints, not shown in the table), the relationship between institutional constraints and

FIGURE 1

Critical and Noncritical Cases from the Perspective of the Normative and Structural Models of Democratic Peace

Case #	Dyad's Attributes		Prediction of Normative Model	Prediction of Structural Model	Type of Case
	Level of Democratic Norms	Level of Political Constraints			
1	Low	Low	Conflict	Conflict	Noncritical
2	Low	High	Conflict	Low Conflict	Critical
3	High	Low	Low Conflict	Conflict	Critical
4	High	High	No Conflict	No Conflict	Noncritical

dispute involvement and dispute escalation ceases to be statistically significant.

This initial bit of evidence suggests that institutional constraints may not be as good an explanation for the lack of conflict between democracies as are democratic norms. However, this is not sufficient for determining that the structural model of democratic peace is outperformed by the normative model. We must move to the critical test.

The critical test examines the differences in the probabilities of conflict in the cases denoted by Figure 1, rows 2–3 (low level of norms with high level of political constraints, vice versa). If the probability of conflict in the case denoted by row 2 is significantly lower than the probability of conflict in the case denoted by row 3, then the structural model is judged superior to the normative one. If the reverse, then the normative model can be said to provide a superior account of the data. Should the difference between them not be statistically significant, then the critical test would be inconclusive.

We also control for democracy in each of the two critical cases to examine whether, beyond the explanation of the model itself, some interaction of political constraints or political stability with democracy takes place. Both models imply that the relationship between political constraints/democratic norms and conflict behavior is independent of whether the states are democracies. If this does not hold and the introduction of democracy significantly alters the relationship between the independent and the dependent variables, then one can argue that the relationship of the critical variable derived from a specific model is spurious.

In order to enable a focused analysis of which model provides a better account of the data in critical cases, we conducted a set of log-linear analyses of the dichotomized versions of the independent variables (stability, executions, and the COPDAB domestic conflict data), using multiple indicators of democratic norms. We first do our analyses only with measures of norms and institutional constraints as independent variables; then we control for democracy (dichotomized) to see if it made a separate contribution.

Table 3 provides the parameter estimates from these analyses. This table shows that generally speaking, the normative model is related to the log odds first for conflict and then for war involvement in all of the cases and with all three measures of political norms. In the table as a whole, the normative constraints are significant in all but two of the 24 cells, whereas institutional constraints are significant in the correct (negative) direction in a minority (11 cells). Controlling for regime type eliminates many of the previously significant parameter estimates of political constraints, but not those for norms, and democracy itself is always significant, even in the crisis (ICB) column.

TABLE 3

The Effects of Democratic Norms, Institutional Constraint, and Regime Type on Conflict Involvement and War Involvement

INDEPENDENT VARIABLE	MILITARIZED DISPUTES	INTERNATIONAL CRISES
	Effects on Conflict Involvement	
Stability	−.401 (.058)**	−.525 (.110)**
Constraint	−.211 (.051)**	−.223 (.089)**
Executions	.416 (.041)**	.391 (.068)**
Constraint	−.089 (.056)	−.108 (.095)
Domestic conflict	.178 (.045)**	.191 (.079)**
Constraint	−.100 (.058)	−.129 (.105)
	Conflict Involvement, Controlling for Regime Type	
Stability	−.306 (.058)**	−.437 (.110)**
Constraint	.013 (.052)	−.025 (.091)
Democracy	−.985 (.128)**	−.900 (.215)**
Executions	.339 (.041)**	.322 (.068)**
Constraint	.124 (.057)*	−.087 (.097)
Democracy	−1.031 (.146)**	−.864 (.218)**
Domestic conflict	.131 (.044)**	.146 (.079)
Constraint	.109 (.059)	−.077 (.107)
Democracy	−1.037 (.157)**	−1.010 (.300)**
	Effects on War Involvement	
Stability	−1.528 (.503)**	−.709 (.231)**
Constraint	−.977 (.293)**	−.739 (.231)**
Executions	.715 (.127)**	.426 (.118)**
Constraint	−.795 (.298)**	−.591 (.236)**
Domestic conflict	.511 (.156)**	.245 (.149)
Constraint	−.674 (.299)*	−.667 (.300)*
	War Involvement, Controlling for Regime Type	
Stability	−1.504 (.503)**	−.665 (.230)**
Constraint	−.790 (.293)**	−.508 (.230)*
Democracy	−4.750**	−4.927**
Executions	.657 (.126)**	.364 (.116)**
Constraint	−.585 (.295)*	−.335 (.233)
Democracy	−4.716**	−4.991**
Domestic conflict	1.054 (.155)**	1.314 (.149)**
Constraint	−.468 (.297)	−.429 (.299)
Democracy	−4.926**	−4.968**

Note: N = 26,129 (stability); 22,870 (executions); and 16,254 (domestic conflict). Entries are unstandardized parameter estimates in log-linear regression equations; standard errors are in parentheses except where standard error cannot be estimated due to zero value in one category of the dependent variable. Chi-squared statistics are infinite.
*$p < .05$.
**$p < .01$.

The relationship of the structural model to conflict occurrence is not nearly as robust as the normative results. Institutional constraints do prevent escalation to war, but they do not prevent states from entering into lower-level disputes—engaging in the kind of lower-level bargaining behavior that conveys toughness and commitment.[22] They may in fact encourage it so long as each side knows that its adversary will be tightly constrained from escalating the dispute all the way up to war. Normative restraints, on the other hand, help to prevent even the emergence of conflicts. Insofar as democracies only rarely engage in

TABLE 4

Critical Test of the Effects of Democratic Norms, Institutional Constraints, and Regime Type on Conflict Involvement

MEASURE OF DEMOCRATIC NORMS	COMBINATIONS OF INDEPENDENT VARIABLES	PROBABILITY OF DISPUTES	PROBABILITY OF CRISES
Stability (N = 26,129)	Low norms, high constr.	2.89%	.95%
	High norms, low constr.	2.11%	.56%
Z-score		−2.07**	−1.87**
Executions (N = 22,870)	Low norms, high constr.	5.71%	1.95%
	High norms, low constr.	2.27%	.82%
Z-score		−3.76**	−2.08**
Domestic conflict (N = 16,262)	Low norms, high constr.	3.96%	1.20%
	High norms, low constr.	1.97%	.38%
Z-score		−5.37**	−4.51**
		Controlling for Regime Type	
Stability			
Not both democracies (N = 22,292)	Low norms, high constr.	3.82%	1.29%
	High norms, low constr.	2.16%	.58%
Z-score		−3.56**	−2.68**
Both democracies (N = 3,837)	Low norms, high constr.	.95%	.03%
	High norms, low constr.	.00%	.00%
Z-score		−3.32**	−1.73*
Executions			
Not both democracies (N = 19,577)	Low norms, high constr.	5.91%	2.12%
	High norms, low constr.	2.48%	.89%
Z-score		−3.58**	−2.08**
Both democracies (N = 3,293)	Low norms, high constr.	.25%	.00%
	High norms, low constr.	.00%	.00%
Z-score		−1.01	—
Domestic conflict			
Not both democracies (N = 14,345)	Low norms, high constr.	3.31%	.64%
	High norms, low constr.	4.10%	1.24%
Z-score		1.34	2.16**
Both democracies (N = 1,917)	Low norms, high constr.	.85%	.15%
	High norms, low constr.	.00%	.00%
Z-score		−3.33**	−1.42

Note: Z-scores represent a difference of proportions test. Negative scores imply that the normative model provides a better explanation than does the structural model; positive scores imply that the structural model provides the superior explanation.
*$p < .05$.
**$p < .01$.

such conflicts, normative restraints seem to deserve the greater credit.

Tables 4 and 5 use information from the Table 3 analyses in the critical test format, to give a sense of how the models perform. The bottoms of the tables also show what happens when the joint regime type for each dyad is controlled for. Columns 1 and 2 in each table represent the occurrence of conflicts and the occurrence of war, respectively. For simplicity, we omit the individual cells and show just the standardized estimates of effects.

Table 4 shows the differences in the probabilities of conflict involvement in the critical cases, and Table 5 does the same for war. They compare the frequency of involvement (both dispute and crisis data) by pairs of states with the combination of low normative constraints and high institutional ones versus high normative and low institutional. They strengthen the previous impression regarding the relative superiority of the normative explanation over the structural–institutional one. In 16 of the 30 separate tests in the two tables, the probability of involvement when the level of democratic norms is high and the level of political constraints is low is significantly below the probability of involvement in the reverse case (with only one test significantly the other way). As before and as expected, the difference almost always appears for conflict involvement in general, much less often for war involvement. The bottom of Table 4, controlling for regime type, shows clearly that three different measures of democratic political norms usually significantly reduce the probability of conflict in dyads, even when the institutional constraints on the regimes are low and even when at least one member of the dyad is not democratic.

These results suggest that the normative model

TABLE 5

Critical Test of the Effects of Democratic Norms, Institutional Constraints, and Regime Type on War Involvement

MEASURE OF DEMOCRATIC NORMS	COMBINATIONS OF INDEPENDENT VARIABLES	PROBABILITY OF DISPUTES	PROBABILITY OF CRISES
Stability (N = 26,129)	Low norms, high constr.	.08%	.14%
	High norms, low constr.	.03%	.15%
Z-score		−.96	.09
Executions (N = 22,870)	Low norms, high constr.	.15%	.20%
	High norms, low constr.	.20%	.30%
Z-score		.35	.58
Domestic conflict (N = 16,262)	Low norms, high constr.	.56%	.42%
	High norms, low constr.	.00%	.00%
Z-score		−6.18*	−5.30*
		Not Both Democracies	
Stability (N = 22,292)	Low norms, high constr.	.12%	.21%
	High norms, low constr.	.03%	.15%
Z-score		−1.21	−.49
Executions (N = 19,577)	Low norms, high constr.	.16%	.00%
	High norms, low constr.	.22%	.00%
Z-score		.38	—
Domestic conflict (N = 14,345)	Low norms, high constr.	.58%	.09%
	High norms, low constr.	.00%	.00%
Z-score		−6.18*	−2.45*

Note: *Z-scores represent a difference-of-proportions test. Negative scores imply that the normative model provides a better explanation than does the* structural model; positive scores imply that the structural model provides the superior explanation. There are no entries for effects on war involvement between democracies because there were no such wars.

*$p < .01$.

provides a more robust and consistent fit to the data than the structural one. The former model has a consistent relationship with both conflict occurrence and war occurrence, almost irrespective of the specific measure of democratic norms used, whereas the latter model sometimes provides a significant relationship, but often not. Moreover, in the critical situations (when one model suggests high levels of conflict and the other suggests low levels of conflict), the predictions of the normative model are more consistent with the data.

CONCLUSION

We have offered a comprehensive analysis of potential explanations of the democratic-peace phenomenon. We draw four conclusions:

1. The democratic peace phenomenon, that is, the relative lack of conflict and complete absence of war between democracies, is probably not a spurious correlation. When controlling for other potentially confounding factors, regime type has a consistent dampening effect on international conflict.
2. These results are robust. They usually hold regardless of the conflict data set used, the definition of the dependent variable, and the scale and type of measure of democracy. This increases our confidence in the substantive results.
3. Both political constraints and democratic norms provide reasonably good explanations of why democracies rarely fight each other.
4. However, the relationship between institutional constraints and measures of dispute and war occurrence is not as robust as the relationship between measures of democratic norms and the dependent variables. This suggests that the normative model may be a better overall account of the democratic-peace phenomenon than the structural model.

Both the fact that the democratic-peace phenomenon is causally meaningful and the fact that we are beginning to move toward a substantive understanding of its causes carry important theoretical implications. First, they suggest that domestic political processes and structures significantly affect state behavior and that these effects are quite generalizable. Second, they provide strong evidence that the strict top–down or outside–in models developed by system theorists are in deep trouble.

In terms of processes operating in the present interstate system, this result suggests that to the extent that norms and institutions take time to develop, newly created democracies in Eastern Europe and elsewhere may still experience some significant amount of interstate conflict while their political systems are in the process of transition to democracy. But the process of global democratization may carry long-term prospects of international stability that arises not out of the missile launchers but out of popular control of governments and of norms of peaceful resolution of political conflicts associated with democratic political systems.

It is possible that major features of the international system can be socially constructed from the bottom up; that is, norms and rules of behavior internationally become extensions of the norms and rules of domestic political behavior. When many states are ruled autocratically (as they were at the Peace of Westphalia and throughout virtually all of history since then), playing by the rules of autocracy may be the only way for any state—democracy or not—to survive in Hobbesian international anarchy. But if enough states become stably democratic—as may be happening in the 1990s—then the possibility emerges of reconstructing the norms and rules of the international system to reflect those of democracies. A system created by autocracies may be recreated by a critical mass of democratic states.

Notes

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1. Axelrod's (1984, 1986) work on the evolution of cooperation and the evolution of norms suggests that norms of behavior are dependent upon the environment in which they are applied. If a conditionally cooperative strategy such as tit-for-tat is confronted by a noncooperative strategy, it would confront the latter on its own noncooperative ground. In fact, short of teaching cooperation to "meanies"—which takes a long time—noncooperative strategies typically force cooperative strategies to become noncooperative. See also Behr 1980; Dacey and Pendegraft 1988.

2. Other writers (e.g., Rummel 1979, vol. 4; Rummel 1983) include elements of both the normative and structural models. Lake (1992) theorizes that any structurally constrained state should be less warlike and imperialistic. His argument should apply to various strong structural constraints by central or federal institutions, whether the state is democratic or not.

3. One inconclusive effort to test them in the modern system is Morgan and Schwebach 1992; on other political systems, see Ember, Ember, and Russett 1992; Russett and Antholis 1992.

4. For a more elaborate discussion of the presumed relationship between these three factors and democratic peace, see Maoz and Russett 1992.

5. An indirect alliance refers to a case where states A and C do not have an alliance with one another but both are aligned to state B. The inclusion of indirect alliance as a constraint on war stems from the structural realists' arguments that states with a common enemy tend not to fight one another (e.g., Mearsheimer 1990, 50–51).

6. In the analysis, the actual number of cases is often much lower due to missing data for some variables and years.

7. These levels are *no dispute, threat of force, display of force, use of force,* and *war* (Gochman and Maoz 1984, 587).

8. For the entire 1800–1986 period, the correlation between democracy and autocracy is $r = -.70, p < .001$; for the 1946–86 period, $r = -.74, p < .001$.

9. Adding 1 prevents division by 0 when the two states have identical scores.

10. $\text{Tau}_b = .48$, gamma $= .58$; $p < .001$, $N = 30{,}049$.

11. Because one of the variables used to produce the institutional constraints index was instrumental in producing the democracy–autocracy index in the original classification (Gurr, Jaggers, and Moore 1989), we expected to find some correlation between the degree of constraints and the degree of democratization. But because other elements also determined both measures, the empirical association is only moderate ($r = .76, p < .001$ for the two continuous measures; $\text{Tau}_b = .72, p < .001$ for the categorical versions). This allows us to use the two measures in the same analysis without serious problems of multicollinearity. We assessed the validity of this measure by correlating it with Morgan and Campbell's (1991) three separate indicators: method of executive selection, decisional constraints, and degree of political competition. The correlations were tau_c and gamma equal to .46 and .72, .63 and .80, and .51 and .66, respectively, with $p < .001$ and $N = 4{,}472$ nation-years.

12. Data are available only for the period 1948–82, restricting the number of years that can be analysed with this measure. These data tend to overreport political violence in democracies and other states in the "center" of the world political system relative to that in nondemocratic and peripheral states (in which information may be suppressed and to which the press gives less attention). Also, highly repressive states are able to prevent much manifestation of antiregime violence. See Duvall and Shamir 1980.

13. Cooperative events were scaled as COOP = 8 eventtype. Conflictual events were added up as CONFLICT = −1(8 − event). This enabled assignment of high cooperative values to the most cooperative events and high conflictual values to highly conflictual events. COPDAB data are available only for 1948–78.

14. Since our economic data cover only the period 1950–84, the dyad years available for analysis with this variable (and for wealth) are fewer than those for which we have conflict data. This variable is responsible for a particularly large number of missing cases due to the temporal averaging of growth levels, which requires three valid annual data points for each state.

15. Revised and updated COW alliance data were provided to us by Allan Ned Sabrosky of Rhodes College. Maoz also updated and refined the COW data from the appendix to Oren 1990.

16. We used these data as reported in Maoz and Russett 1992. The original COW data set ended in 1982; Maoz cleaned and updated it to 1986, then checked it with a parallel cleaning and updating by Charles Gochman (whose data, and a similar typology, are used in Bremer 1992).

17. To the extent that any of the nonregime factors examined in the first stage was found to have significant effect on the probability of dispute in the dyad, this factor was controlled for at this point also. This was done to ascertain that the relationships between any one of the two models and dispute involvement or escalation were not spurious.

18. Bruce Bueno de Mesquita, personal communication, 1992; cf. Bueno de Mesquita and Lalman 1992, 281–82.

19. We thank Christopher Achen for advice on dealing with this problem.

20. Regarding the use of logistic analysis on polychotomous dependent variables, see Fienberg 1980 and Hosmer and Lemonshow 1989. The results in some tables differ slightly from those computed earler and reported by Maoz and Russett in Russett 1993, chap. 4.

21. The growth variable is not included in this set of analyses due to the large number of missing cases that it generates. Specifically, the introduction of the growth measure along with the domestic political conflict or the political-executions variables in one equation reduces the number of dispute dyads by 65%, thereby considerably distorting the distribution of the dependent variable.

22. Another set of analyses was performed using the deaths from political violence as an indicator of democratic norms. This set yielded basically the same results as those shown in the table. The same is true for the results reported in Table 5.

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ENDOGENOUS PREFERENCES AND THE STUDY OF INSTITUTIONS

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The assumption that individual preferences, or attitudes, are fixed and exogenously determined is central to many studies of political and economic institutions, such as markets and elections. We present a Bayesian model of adaptive preferences and empirical evidence consistent with that model to argue that preferences are not always exogenous and fixed. The changing relationships between partisanship and preferences on civil rights issues between 1956 and 1964 and on the Vietnam War issue between 1968 and 1972 coincide with significant changes in the major parties' positions on these issues, suggesting that preferences are endogenous to the electoral process. We conclude with a discussion of the positive and normative implications of endogenous preferences.

The assumption that individual preferences, or attitudes, are fixed and exogenously determined is central to many studies of political and economic institutions, such as markets and elections. In positive studies, preferences are often the starting point for analyzing how variations in the rules and structures that define institutions lead to different collective results. For example, positive theories of electoral systems model nonpartisan, two-party, and multiparty systems and compare the winning platforms produced by each system. In normative studies, these collective results are evaluated relative to the distribution of preferences. Systems expected to produce stable outcomes close to the mean preference are often preferred to those with outcomes that are unstable or in a tail of the distribution. (On the positive and normative points, see Riker and Ordeshook 1973.) The assumptions of exogenous and fixed individual preferences are critical to both sets of analyses.[1]

We present empirical evidence that preferences are not always exogenous and fixed. Individual preferences may be altered by actions taken during the normal sequence of events associated with institutional processes. In the current analysis, we explore the possibility that voters change their preferences in response to shifts in the positions of competing parties in successive elections. The relations between voter preferences and party platforms on civil rights between 1956 and 1964 and on the Vietnam War issue between 1968 and 1972 provide the empirical basis for this study. We extend the traditional spatial electoral model to include endogenous preferences, then we describe the estimation procedures and present the empirical analysis of party competition and the endogeneity of policy preferences. Finally, we discuss the positive and normative implications of making individual preferences endogenous to the study of institutions.

THE TWO-PARTY ELECTORAL MODEL

The study of electoral competition offers a powerful setting in which to examine the concept of endogenous preferences. We begin with the traditional spatial model of two-party electoral competition developed by Downs (1957) and substantially refined by Hinich and his many coauthors (see Davis and Hinich 1967, 1968; Davis, Hinich, and Ordeshook 1970; Enelow and Hinich 1981, 1982, 1984a, 1984b, 1989; Hinich 1977, 1978). The major features of this model are well known: with a single issue dimension and two competing parties, the central tendency of the distribution of voters' preferences emerges as the long-run stable equilibrium. There is considerable empirical support for the prediction that partisanship and vote choices are related to the relative distance between voters' preferences and the positions adopted by party leaders and candidates (Franklin and Jackson 1983; Jackson 1975a, 1975b, 1992a; Rivers 1988; see also the literature summarized in Beck 1986).

We suggest that parties and their electoral strategies help shape individual attitudes and behavior. Voters take cues from the electoral participants as they develop their preferences. This means that voters perceive party positions and hear the debate surrounding the adoption of those positions and alter their own political attitudes in response. Carmines and Stimson (1989) present a powerful illustration of how the actions of party leaders led to changes in civil rights preferences during the late 1950s and early 1960s. Zaller (1991b) argues that the cues provided by political elites, who include (but would not be limited to) party leaders, were important forces in changing opinions on Vietnam War policy between 1964 and 1970. Our argument is much more than an assertion that voters do not in fact have fixed preferences. In the model we present, voter preferences shift in direct response to the actions of the parties as they search for strategies to increase their likelihood of winning elections. It is in this very specific sense that we talk about preferences being endogenous in the model of electoral competition.

The model we propose is a variant of the models of adaptive utility developed by Cyert and DeGroot (1987) and Cohen and Axelrod (1984). In these models, an individual's decision making is described as a Bayesian process. Faced with any choice, people calculate their expected utility based on their prior beliefs about the possible utilities for each outcome. Once they receive new information, such as occurs after a choice, they update their utility function based on how much they liked the outcome *relative* to their prior. If there is a significant difference between the actual and the anticipated utility (i.e., a surprise), people then update their expected utility to incorporate this new information. In the Cyert–DeGroot and Cohen–Axelrod models, this new information comes from actually experiencing the result of a choice, as when a consumer buys an item, and the updated utility distribution can be applied to the next decision where that particular choice is also present.

Our use of the adaptive utility model differs in one significant way from the models just described. In the voting case, voters have uncertain utilities for the programs being debated publicly. This uncertainty arises, in part, from not knowing a policy's consequences, but as Cyert and DeGroot argue, this uncertainty also arises because people do not know how much they will like an outcome if and when it does occur.[2] As the parties propose and advocate different policies, citizens acquire information about possible outcomes and about how much other people like these policies. For most citizens, some of these other people are like themselves and "represent" them (e.g., members of an individual's own party), while others are different and do not represent them (e.g., members of the opposition parties). Shifts in party positions provide the surprises described in the adaptive utility models and may cause people to update their utility distributions (and thereby their expected utility) for the different programs being debated by the parties. The larger the shift in either party's position, the bigger the surprise and the greater the shift in expected utilities and in individual preferences. Bartels (1993) uses a similar scheme to analyze the effect of media coverage of campaigns.

To test this hypothesized link between partisanship, party platforms, and preferences empirically, we will look for evidence that changing party positions have an independent effect on the attitudes of partisans. If the effect is important, it suggests that as parties change their policy positions in their search for electoral majorities, individual partisans change their policy preferences, and the aggregate preference distribution changes. Positive findings on this relationship, combined with the earlier results deriving individuals' party identification and votes from the party positions, means that preferences and outcomes are jointly determined.

FORMALIZATION

We first present a formalization of the two-party electoral model that permits individual preferences to be altered by changes in the parties' position choices. The model includes equations that represent vote choices, party positions, and voter preferences. The first four equations represent the traditional spatial model where individual preferences, Y_i, are fixed, which implies that $\bar{Y}$ and σ_y^2 are constant. A voter's electoral preference is denoted by v_i and the actual vote choice by V_i. The two parties are denoted as Theta and Phi and their respective policy positions by θ and ϕ. Voter's preference functions take the form of a quadratic loss function common in spatial models (and estimated empirically in Erikson and Romero 1990; Jackson 1992a; Rivers 1988).[3]

$$\begin{aligned} v_i &= -\alpha[(Y_i - \theta)^2 - (Y_i - \phi)^2)] + u_{1_i} \\ &= 2\alpha(\theta - \phi)Y_i - \alpha(\theta^2 - \phi^2) + u_{1_i} \qquad (1) \end{aligned}$$

$$\begin{aligned} V_i &= \theta \ \mathit{if}\ v_i > \lambda \\ &= 0 \ \mathit{if}\ -\lambda \leq v_i \leq \lambda \\ &= \phi \ \mathit{if}\ v_i < -\lambda \qquad (2) \end{aligned}$$

$$\theta = f_1(\bar{Y}, \sigma_y^2, \phi) \qquad (3)$$

$$\phi = f_2(\bar{Y}, \sigma_y^2, \theta) \qquad (4)$$

Equations 1 and 2 indicate that a voter's preference for each party is a function of the squared difference between the individual's policy preferences and the positions proposed by the two parties plus a stochastic term. If the difference between the individual's preference and Theta's position is less than the difference between the individual's preference and Phi's position, then the vote choice is Theta. If the difference in positions is within a very small range around zero, $[-\lambda, +\lambda]$, the voter sees no difference between the parties and abstains.[4] Equations 3 and 4 indicate that party strategies are a function of the preference distribution and the other party's platform. In the long run, both parties will move to $\bar{Y}$—or a point close to it—as the dominant strategy.

The traditional model is usually expanded to include an equation for party identification. As parties shift their policy positions in competition for votes, individuals may realize that their relative proximity to each party has changed and they may alter their partisanship in response. To capture this connection between partisan affiliation and changing party positions, we model individual partisanship at time *t* as a weighted average of partisanship at time *t* − 1 and the relative differences between the individual's own policy positions and those of the two parties. (For other work consistent with this conceptualization of partisan evolution, see Achen 1992; Erikson and Romero 1990; Franklin and Jackson 1983.) While this additional equation does not affect the basic dynamic of the model of party competition, it adds a richness consistent with empirical observations of what parties do and how voters respond to the parties' behavior. The model of party identification, denoted by P_{it} can be expressed formally as

$$\begin{aligned} P_{it} = \delta P_{i,t-1} + (1 - \delta)\{-\alpha[(Y_i - \theta)^2 \\ - (Y_i - \phi)^2]\} + u_{2_i}. \end{aligned}$$

We make a major alteration to this model by adding an equation to capture the process by which individuals develop their policy preferences. Equation 5 relates individual preferences to shifts in the platforms adopted by the two parties. The addition of equation 5 substantially changes the dynamics of the model, but we believe it better represents empirical reality. The adaptive utility preference model is

$$Y_{it} = Y_{i,t-1} + X_{it}A_1 + \Gamma_1[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})]P_{it} + u_{3_i}. \quad (5)$$

Y_{it} is individual i's issue preference in the current election period, t. $Y_{i,t-1}$ denotes preferences carried over from the previous period, that is, individuals' preferences based on their Bayesian priors. $X_{it}A_1$ denotes the relationship between preferences and individual exogenous attributes, such as income, race, age, residential location, etc., and is included to account for any exogenously derived preference change. Equation 5 implies that as the parties' platforms change, individual preferences will move with the platform change of the party with which they identify and opposite to the platform change of the opposition party.[5] For example, if $(\theta_t - \theta_{t-1}) > 0$ and $(\phi_t - \phi_{t-1}) < 0$, voters who identify with party Theta will adjust their preferences in a positive direction, while Phi identifiers will develop more negative preferences. If $\Gamma_1 = 0$ and $\sigma^2_{u_3} = 0$, the model reduces to the traditional spatial model with exogenously given preferences. In this traditional model, preferences remain fixed unless there is some exogenous change, represented by $A_1 \neq 0$. We estimate the term $\Gamma_1[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})]$ for cases where $[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})] \neq 0$.

PARTY PLATFORMS AND PREFERENCES

Equation 5 must be altered slightly to test empirically whether individual preferences are related to party positions. First, we are unable to measure actual party positions directly, so we do not know $[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})]$. Second and more serious, these terms are constant across individuals in any election study, except for small variations due to shifts in positions during a campaign or to ambiguity perpetrated by the candidates and parties themselves. As Kramer (1983) has argued, these types of cross-sectional variations are not appropriate for assessing the role of factors that primarily vary across elections.

The strategy followed here is to estimate the relationship between individuals' partisanship and preferences for a sequence of elections during which there are large observable changes in party positions on a given issue. In formal terms, we estimate the model,

$$Y_{it} = Y_{i,t-1} + X_{it}A_1 + C_1P_{it} + u_{3_{it}}, \quad (6)$$

where $C_1 = \Gamma_1[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})]$. C_1 is constant across individuals for any single election but will vary across elections, depending upon the shifts in party positions from one election to the next.

In the cases we examine, the changes in party positions were quite dramatic from one election to the next, and in one case, the parties even reversed themselves over two election intervals. Thus $[(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1})]$ both is large and its sign known, implying that C_1 should be large and have the sign predicted by the changes in party positions and should reverse sign when the party positions are reversed. The test of the adaptive utility model, then, is whether we observe changes in the relationship between partisanship and preferences from one election to the next that correspond to shifts in party positions. We shall describe these shifts in party positions.

The Parties and Civil Rights: 1956–1964

The first empirical test examines changes in the parties' positions on civil rights between 1956 and 1964 and the impact these changes had on partisan attitudes on civil rights issues. In the mid-1950s, civil rights was not yet a salient national political issue, though with the 1954 Supreme Court decision in *Brown* v. *Board of Education*, the salience was beginning to grow. By 1958, however, school desegregation had become much more salient and differences between the parties were evident. In the Little Rock incident in 1958, a Republican president, carrying out a Supreme Court decision written by a Republican-appointed chief justice, sent federal troops to integrate the local schools by force. This action was strongly opposed by the Democratic governor of Arkansas.

These events offered cues that Republican leaders favored federal civil rights actions. The positions of Democratic leaders offered more mixed cues, though the Democratic congressional leaders vigorously opposed civil rights legislation. These partisan differences diminished slightly during the 1960 presidential election. Late in his brother's campaign, Robert Kennedy worked to have Martin Luther King released on bail from the Georgia penitentiary and John Kennedy expressed his concern in a call to Mrs. King. However, the Democratic congressional leaders' opposition to civil rights legislation was unabated. Carmines and Stimson (1989) offer detailed evidence on the relative party positions, using measures of congressional roll-call voting and party platforms. They show quite unambiguously that through 1960 the Republicans, as a party, were more pro-civil-rights than were the Democrats, as a party. Dawson (1992) offers a similar assessment of the relative positions of the two parties during this period.

By the 1964 campaign, positions and events had changed dramatically. Presidents Kennedy and Johnson had championed the Civil Rights Act of 1964, which GOP presidential candidate Barry Goldwater strongly and visibly opposed. The issues of federal guarantees of equal access to jobs and housing were by then a central part of the bill and the debate surrounding it. Voters clearly perceived these positions. In the 1964 National Election Studies (NES) postelection interviews, people were asked whether they knew of the 1964 Civil Rights Act and what positions Johnson and Goldwater had taken. Three-quarters (77%) knew of the act, of whom 96% said Johnson supported it and 84% said Goldwater opposed it. Events indicate that party leaders had dramatically shifted positions and that these shifts were perceived by the electorate.

The Parties and the Vietnam War: 1968–1972

The second case we examine in which the parties dramatically shifted positions is on the policy to be pursued in Vietnam between 1968 and 1972. In 1968, the war was clearly identified as the Democrat's war, having begun in the Kennedy administration and then dramatically and tragically escalated by Johnson after 1965. There were Democrats who opposed the effort, but the 1968 convention indicated quite clearly that they did not control the party. The Democrat's position changed significantly by 1972, however, with the emergence and ultimate convention victory of George McGovern. These events left no doubt that by 1972 the Democratic party leadership was solidly opposed to the involvement in Southeast Asia.

The early Republican positions were somewhat less clear. Neither candidate Nixon nor the party took a strong antiwar position during the 1968 campaign, though Nixon tried to convince people that he had a plan for ending the war. (On Nixon's early stance towards the war, see Herring 1986.) In part, given the actions of the Johnson administration and Humphrey's positions throughout most of the campaign, the Republicans did not need to take an explicit position to appear to be less hawkish. By 1972, all this had changed. Nixon had not ended the war; in fact, he had pursued it with substantial vigor during his first four years. Thus, we have a second case in which the parties' relative positions changed substantially over a short period of time.

The civil rights and Vietnam War periods provide the rare (for voting research) natural experiment in which, over a relatively short period of time, the parties dramatically shifted their positions, both in absolute terms and relative to each other. This is precisely the situation needed to test the proposition that the actions of the parties, in setting their positions, influence individual preferences. If changes in the parties' positions do influence preferences, then we ought to see predictable values of C_1. Between the 1956 and 1960 elections, Republicans should become more pro-civil-rights, and Democrats, less so, for given values of $X_{it}A_1$. Between 1960 and 1964, the relationship between party identification and preferences should reverse, with Democrats becoming more pro-civil-rights. As to the Vietnam War, Republicans should become more hawkish, and Democrats more dovish, between 1968 and 1972. If we do not observe these changes in the relationship between party identification and preferences and the corresponding values of C_1, it creates considerable doubt about the endogeneity of preferences.

Data and Measures of Preferences and Partisanship

Data to estimate C_1 for the relevant years comes from the National Election Studies. The civil rights data comes from the first American Panel study done in 1956, 1958, and 1960 and the 1964 election study. Two civil rights preference questions were asked in these studies, one related to school integration and the other to equal treatment in jobs and housing (1956–60) and jobs only (1964). The Vietnam data come from the 1968, 1970, and 1972 election studies. In 1968 and 1970, people were asked what we should do in Vietnam and were given the options to pull out entirely, to keep our soldiers there but try to end the fighting, or to take a stronger stand. Also in 1970 and then again in 1972 respondents were asked to place themselves on a seven-point scale where one end represented those who thought we should withdraw from Vietnam immediately, and the other indicated those who wanted a complete military victory.

Coding of the preference variables is slightly complicated. In 1956 and 1960, the election studies used a five-item Likert scale to measure respondents' agreement or disagreement with the civil rights policy questions. In 1964, the format was changed to a three-point scale asking respondents their preference or indifference between two opposite policies. In order to make the structure of the questions comparable for all three years, the five-point scales from 1956 and 1960 were collapsed to three-point scales measuring *agree*, *disagree*, or *no difference*. This procedure entails very little loss of information, since most people chose the more extreme strong agree or disagree options.

Partisanship is measured with the traditional seven-point scale, with strong Democrats recoded as +1, pure Independents as 0, and strong Republicans as −1 and is treated as an interval variable.

ESTIMATION PROCEDURES

Estimation of C_1 in equation 6 presents numerous problems. The estimation methods that we shall describe address each of these problems.

Exogenous Preference Changes

One important aspect of the model in equation 6 is the inclusion of the exogenous variables X_{it} to capture systematic changes in preferences attributable to factors other than partisanship, such as the actions of interest groups or particular events. Events that shift the preferences of one set of people relative to another set (e.g., blacks relative to whites, younger relative to older voters) are captured by the coefficients in A_1 for the respective variables, which are

TABLE 1

Exogenous Variables and Equation Specification

VARIABLE	PARTY	CIVIL RIGHTS	VIETNAM WAR
Father's ID	x		
Pre-1932 cohort	x		
% Democrat vote[a]	x		
Log density[b]	x		
Log county income[b]	x		
% Black[b]	x	x	
% Urban[b]	x	x	
Age	x	x	x
Education	x	x	x
Log income	x	x	x
Male	x	x	x
Black	x	x	x
South	x	x	
Catholic	x	x	
Jewish	x	x	x
Union member	x		x
Age * education	x[c]		x

Note: x indicates where variable is included.
[a] Measured for the respondent's county of residence and for the previous presidential election year.
[b] Measured for the respondent's county of residence.
[c] Only for the model for Vietnam attitudes.

shown in Table 1, columns 2 and 3. The coefficients on these variables will capture any systematic changes in preferences not related to the change in party positions.

Preferences and Changes in Partisanship

The second problem addressed in our estimation procedure, the confounding results created by changes in party platforms leading people to change their partisanship, is addressed by the use of an instrumental variables estimator. This estimator allows us to estimate a reduced-form model of partisanship and to use the predicted value of partisanship as an estimate of partisanship unaffected by preferences and changes in party platforms. This procedure is analogous to a two-stage least-squares technique with interval measures. A set of the variables shown in Table 1 are expected to be related to partisanship but not directly to civil rights and Vietnam policy preferences as indicated by the *X*s in the respective columns. Two of these variables are individual-level measures assessing the respondent's father's party identification and whether the respondent is in the cohort that entered electoral politics prior to the Roosevelt victory in 1932. The remaining variables measure the likely partisan character of the county in which the respondent resides. Respondents in areas where the Democrats (Republicans) are more numerous, stronger, and more active may be influenced by the local partisan context and are more likely to be Democrats (Republicans) themselves. Thus, we expect that respondents in areas with a larger Democratic vote, or that are more urban and more densely populated, or that have a larger proportion of black residents, or that have more poor families are more likely to be Democrats.

Cross-Sectional Data

Our adaptive utility model is explicitly a model of preference change. We are interested in how individuals update their prior policy preferences in response to changes in the positions taken by the parties. To capture these changes, we need measures of preferences at time t and at time $t - 1$. Ideally, we would use panel data to trace an individual's preference change over time. However, a panel design is infeasible because panel data are available only for two years in our period of study, and so we must rely on cross-sectional data from each year and make estimates of the individual-level preference change.

To estimate preference change, we employ a technique developed by Franklin (1989) called *two-stage auxiliary instrumental variables*. This estimator allows us to simulate panel data from consecutive cross sections when similar data are collected in several years (but from different respondents). As the name suggests, the technique involves a two-stage estimation procedure. In the first stage, we estimate a model of individual preferences for the sample interviewed at time $t - 1$. Preferences (in our case, preferences towards civil rights or the Vietnam War) are related to a set of explanatory variables (e.g., individual-level demographic and contextual variables) also measured at $t - 1$. In the second stage, we use the coefficients estimated in the first stage and calculate a predicted value of $t - 1$ preferences for time t respondents using the explanatory variables measured at time t. (For an extended discussion of this method and its underlying assumptions, see Franklin 1989.) This method was used to model change in partisanship for periods without panel data and to model changes in Vietnam preferences (Franklin and Jackson 1983; Zaller 1991b).

The structure of the response model forces us to follow this procedure even for the years with panel data. Actual preferences in equation 6 are not observed directly, as we shall see. Thus, even with panel data we must estimate preferences at $t - 1$ as indicated and use that estimated value in the model for opinion at time t. The only advantage that panel data provides is knowledge of the values of X_i at time $t - 1$ for the respondents at time t, instead of having to infer them from the values at time t.

Changes in Questions

Our estimation procedures are further complicated by changes in the wording and format of the civil rights and Vietnam War questions. There are persuasive suggestions in the literature that changes in question format and wording may create the appearance of preference change when none exists (see Bishop, Tuchfarber, and Oldendick 1978; Sullivan, Piereson, and Marcus 1978). Our concern is whether these changes alter the relationship between partisanship and the measure of preferences. If changes in question wording and format shift the whole distribution uniformly so that everyone appears to become more liberal or conservative, the changes will not affect our results about the relationship between party platforms and preferences. Only if Republicans but not Democrats (or the reverse) are induced to respond differently will the question changes distort our main results.[6]

According to Sullivan, Piereson, and Marcus (1978) a prime cause of the spurious observed preference change between 1960 and 1964 is the shift from the Likert *agree/disagree* format to a format that asked respondents to choose between two opposite policy options. They suggest that in the former structure respondents who are uncertain about their real opinions, rather than indicate that they do not have an opinion, may be more likely to give an *agree* response, despite efforts to filter them out. Such behavior leads to a response bias correlated with any characteristics that are correlated with not having an attitude. (For additional evidence supporting their contention, see Jackson 1979.) The shift to the second question format changes the likelihood of these response biases, thus possibly giving the false appearance of a shift in preferences.

These problems of changing response bias and induced preference change are also a problem with the Vietnam issue. The NES used two forms of the Vietnam attitude question, overlapping them in 1970. Our concern is whether the switch to a seven-point scale from the three-item categorical measure used in 1968 induces more—or less—response bias. The second concern is whether this wording and format change alters the relationship between measured opinion and partisanship and the other demographic variables. The point of the estimation procedure, then, is to estimate the separate effects of individual preferences and any possible response bias as they are related to responses to the preference questions for each year. To the extent that changes in question wording affect patterns in response bias, we can identify and explicitly model this bias and therefore isolate changes in preferences over time.

The estimation method is based on two equations. One equation, identical to equation 6, models individuals' preferences as a function of their past preferences, $Y_{1i,t-1}$, the exogenous variables, X_{1it}, and their party identification, P_{it}. We denote this equation as

$$Y_{1it} = Y_{1i,t-1} + X_{1it}B_1 + C_1P_{it}.$$

The second equation models respondent *i*'s uncertainty about his or her opinion. The second equation is $Y_{2it} = X_{2it}B_2$, where X_{2it} includes the respondent's age, education, income (logged), gender, and whether the respondent's father was an independent, denoted as find. Our expectation is that the likelihood of not having an opinion is negatively related to education, income, and maleness and positively related to age and having an independent father.

The variables denoted by Y_{1i} and Y_{2i} are unobserved, latent variables, which are imperfectly measured by the preference questions. This type of model resembles the traditional MIMC (multiindicators–multicauses) model, except that instead of continuous observed endogenous variables, we have a categorical measure that is related to these two concepts. We shall explore solutions to this problem. (For an extended presentation of this model, see Franklin and Jackson 1983.)

Limited Dependent Variables

A final complicating factor is that the dependent variables measuring preferences are not the continuous, interval-level variables required to estimate the model using standard linear techniques, such as two-stage least squares or LISREL. In addition, some response options may be categorical, not ordinal. For example, on the three-point Vietnam War policy questions, *proceed as we are now* may be interpreted by the respondent not as a central response between *pull out now* and *complete military victory* but rather may be considered less desirable than the two more active alternatives. The possibility that respondents without preferences gave an opinionated response leads to further deviations from ordinality. A better method to deal with multiple and possibly unordered response categories (which we employ) is a multinomial logistic structure that compares the probabilities associated with any of the possible responses to an arbitrarily chosen base category.

The full statistical model relates the two latent variables, Y_{1i} and Y_{2i} to an individual's responses on the multinomial preference question. (We drop the *t* subscript at this point for expositional clarity.) We denote person *i*'s response to an opinion question by Z_i and each of the categorical choices by $j = 1, \ldots, J$. We are interested in modeling the probability of a respondent choosing category *j* as a function of their underlying preference, Y_{1i}, and their likelihood of having an opinion, Y_{2i}. We can express this probability as

$$Prob(Z_i = j) = \frac{e^{A_{1j}Y_{1i} + A_{2j}Y_{2i} + A_{3j}}}{\sum_{j=1}^{J} e^{A_{1j}Y_{1i} + A_{2j}Y_{2i} + A_{3j}}}. \qquad (7)$$

The expectation is that the probability of giving any one of the opinionated responses, such as *agree* (on the civil rights questions) or that we should withdraw from Vietnam, relative to any of the other opinionated categories is directly related to variations in Y_1 as measured by the A_{1j}'s. The more extreme the values of Y_1, the higher the probability of a corresponding, extreme opinion response. Second, the likelihood of giving an explicit *no opinion* response will be related to the *no opinion* variable, Y_2, and to the preference measure, Y_1. We expect the probability of a *no opinion* response to be highest for values of Y_1 in the indifference or nonextreme range.

Lastly (and importantly for our efforts to measure C_1 accurately), we estimate the relationship between variations in the *no opinion* measure, Y_2, and the probability of a substantive response, such as *agree*. If there is no response bias related to nonattitudes, then the values of all the A_{2j} coefficients will equal zero except in the equation for the explicit *no opinion* category. In this case, as the likelihood of having no opinion increases, as denoted by higher values of Y_2, there would be no change in the probability of respondents choosing one of the opinionated categories relative to any other. Sullivan, Piereson, and Marcus's (1978) argument is that this last probability is not zero *and* that this probability changes with alterations in question structure. Having explicitly included response bias and how it may have changed across years, we can more accurately estimate the independent effects of changes in party positions on preferences. (For a full presentation of this model, including the propositions about no opinions and possible response biases, see Jackson 1992b.)

EMPIRICAL RESULTS

The model just described has several different components, some of which are secondary to our concern with the relationship between party identification and preference change. Consequently, we will summarize these secondary results quickly before reporting the models of preference change described in equation 6.

The need to model the possibility of those with a low likelihood of having opinions confounding the opinion responses with guessing is justified in 7 of the 10 cases.[7] All of the results we shall report therefore include the possibility of response bias. When we model the probability of each response category, the value of the *no opinion* equation is set to its sample mean.

We also tested whether responses to the two different Vietnam questions in 1970 are likely measuring the same latent attitude and *no opinion* equation. The statistical test for whether the two structures are the same could not be rejected at any reasonable probability level. All the results for 1970 attitudes reported here are for this single structure.

We now describe our primary results, the preference model for each issue, equation 6, and the relationship between preferences and the question responses, equation 7. This discussion concentrates on the role of party identification and summarizes the main exogenous effects on preference change. Next, we present the multinomial logit model, giving the probability of each response category as a function of the values of the attitude variable Y_{1i} for a respondent with the mean likelihood of not having an opinion. Lastly, to provide a further perspective on the strength of the relationship between partisanship and preference change, we show the differences in the expected opinion of Democrats, Independents, and Republicans, holding other factors constant.

TABLE 2

Attitudes on Federal Efforts To Integrate Schools

	COEFFICIENTS IN PREFERENCE EQUATION, $Y_{1,t}$		
VARIABLE	1956	1960	1964
Party ID[a]	−.02 (.24)	.63 (.24)	−.73 (.22)
Male	.74 (.83)	−.10 (.59)	−.59 (.18)
Black	−1.50 (.35)	−.44 (.39)	−1.22 (.27)
Age/100	1.65 (1.38)	−.86 (1.96)	−.72 (.54)
Educ/10	.40 (.70)	.15 (1.64)	−1.70 (.52)
log(Inc)	.27 (.42)	.47 (.55)	−.76 (.11)
South	.90 (.23)	−.29 (.23)	.13 (.17)
Jewish	−1.05 (.48)	−1.02 (.57)	.87 (.50)
Catholic	−.26 (.22)	−.39 (.22)	−.27 (.18)
% black	1.00 (.90)	1.85 (1.00)	.18 (.67)
% urban	−.43 (.30)	−.24 (.31)	−.34 (.26)
$Y_{1,t-1}$	—[b]	1	1
σ^c	.857	1.125	1.456

Note: High score indicates opposition to efforts to integrate schools. Asymptotic standard errors are in parentheses.

[a] Instrumental variable for Party ID.

[b] No lagged preferences included.

[c] Standard deviation in preferences.

School Integration

The estimated equations for school integration preferences are shown in Table 2. The columns in this table show the coefficients in equation 6 for 1956, 1960, and 1964 and estimate how we expect preferences to differ among respondents with different characteristics. For example, in 1956 the preferences of strong Democrats and strong Republicans are expected to differ by only −.02 while those of blacks and whites are expected to differ by −1.50, ceteribus paribus. (Remember that a positive value indicates greater agreement with the statement that the Federal government should stay out of the school integration matter.) The equation for 1956 does not include a lagged preference variable because it serves as our base year. The model is constructed so that the mean value for the preference equation is zero. The standard deviation of estimated preferences is included to provide a relative measure of dispersion and of how differences associated with any one variable compare to overall differences.

The expected relationships between party identification and change in school integration preferences are clearly evident in 1960 and 1964. The coefficient of .63 in the equation for 1960 preferences indicates that between 1956 and 1960, relative to 1956 preferences, Democrats became more likely to agree and Republicans to disagree that the federal government should stay out of the question of school integration. Since the party identification variable is scaled from -1 to 1, this coefficient indicates that the expected preference difference between a strong Democrat and a strong Republican in 1960 is $1.26 = .63 * [1 - (-1)]$, other things (e.g., 1956 preferences, the values of the exogenous variables) being equal. (We shall provide a numerical interpretation of this coefficient in the next section.) Between 1960 and 1964, just the opposite shift occurred, as predicted. The coefficient of $-.73$ implies that the expected preference difference in 1964 between a strong Democrat and a strong Republican with the same 1960 preferences in 1.46. In both cases, the coefficients are close to three times the size of their estimated asymptotic standard error.

Several of the exogenous variables were also strongly related to preferences, as expected. Furthermore, the coefficients on these variables generally fit the patterns expected if they are adequately measuring the exogenously determined preference changes. In 1956 blacks relative to whites and Jews (and Catholics to a lesser extent) relative to Protestants were more likely to think that the federal government should integrate local schools, as indicated by the coefficients of -1.05 and $-.26$ on the Jewish and Catholic variables respectively. The coefficient of 0.90 on the South variable shows that southerners were more likely to oppose school integration than nonsoutherners.

The relationships between attitudes on school desegregation and the exogenous variables were relatively stable between 1956 and 1960. The largest preference change between 1956 and 1960 was the increase in opposition to federal integration efforts among respondents residing in areas with a high proportion of black residents in their county. The coefficient of 1.85 indicates that for every 10% increase in the proportion of black residents in the respondents' counties, opposition to school integration increased by .185 over what it was in 1956. This relationship was evident in 1956, where the coefficient is 1.0, but increased sharply by 1960. These results are consistent with Key (1949) and Wright (1977), who argued that the strongest antipathy towards blacks was in counties with large black populations. Between 1956 and 1960 there were also attitude shifts related to religious preference, with Catholics and (particularly) Jews relative to Protestants becoming more supportive of integration efforts.

There were significant exogenously driven changes in school desegregation attitudes between 1960 and 1964. The coefficients on the race, age, education and income variables in particular show these changes. Not surprisingly, blacks relative to whites showed increased support for federal school integration efforts. In 1956 and 1960, there were slight positive relationships between age, income, and education and opposition to forced school integration. Between 1960 and 1964, these relationships reversed direction such that the better educated were more likely to favor desegregation efforts and the relationships between attitudes and age and income had largely disappeared. These results suggest that important exogenously motivated changes in attitudes during this period are being captured by the variables in the model.

TABLE 3

Response Model for Federal Efforts To Integrate Schools

RESPONSE CATEGORY	VARIABLE	COEFFICIENTS IN RESPONSE MODEL 1956	1960	1964
No Opinion				
	Constant A_3	−1.37	−1.23	−1.23
	Preference A_1	.39	.28	.43
		(.17)	(.13)	(.06)
Agree				
	Constant A_3	.06	−.01	−.50
	Preference A_1	1	1	1
Indifferent				
	Constant A_3	−1.78	−2.05	−1.85
	Preference A_1	.55	.40	.47
		(.19)	(.18)	(.09)
Disagree				
	Constant A_3	0	0	0
	Preference A_1	0	0	0

Note: Asymptotic standard errors are in parentheses.

We now relate the expected values of the preference equations to the probability of each response. Table 3 shows the coefficients denoted by the *A*s in equation 7 that relate the preferences estimated in Table 2 to the probability of each response for respondents with an average likelihood of having an opinion, i.e. $Y_{2i} = 0$. In order to normalize and scale the model, certain values of *A* are set to zero and one. First, the zero values in the response equation for the *disagree* response establish this category as the base response. The other coefficients, therefore, give the log-odds of that response relative to a *disagree*.[8] Second, the coefficient relating preferences to the *agree* response is set to one. This scales the preference equation so the coefficients in Table 2 give the difference in the log-odds of an *agree* relative to a *disagree* response associated with differences in each of the variables. For example, in 1960 the log-odds of an *agree* relative to a *disagree* response is 1.26 higher for a strong Democrat than for a strong Republican. Similarly, the log-odds of an *indifferent* relative to a *disagree* response is .504 higher ($.40 * 1.26$) for a strong Democrat than for a strong Republican.

There are two patterns of note in Table 3. The coefficients relating preferences to the probability of each response are very stable in all three years (once we have controlled for any response bias associated

TABLE 4

Calculation of Response Probabilities for School integration in 1960

	$e^{A_{1j}Y_1+A_{2j}\bar{Y}_2+A_{2j}}$					PROBABILITY OF EACH RESPONSE[c]			
Y_1[a]	DISAG.	INDIFF	AGREE	NO OP	SUM[b]	DISAG.	INDIFF	AGREE	NO OP
−3.375	1.000	.034	.034	.111	1.179	.848	.029	.029	.094
−2.250	1.000	.054	.103	.153	1.310	.763	.041	.079	.116
−1.125	1.000	.085	.318	.209	1.612	.620	.052	.197	.130
−.730[d]	1.000	.099	.472	.234	1.805	.554	.055	.262	.129
.000	1.000	.134	.980	.287	2.399	.417	.055	.409	.119
.530[e]	1.000	.164	1.665	.332	3.162	.316	.052	.527	.105
1.125	1.000	.208	3.019	.393	4.620	.216	.045	.654	.085
2.250	1.000	.326	9.300	.538	11.164	.090	.029	.833	.048
3.375	1.000	.512	28.646	.737	30.894	.032	.017	.927	.024

[a] Value of expected 1960 preferences from Table 2.

[b] $\sum_{j=1}^{J} e^{A_{1j}Y_1+A_{2j}\bar{Y}_2+A_{3j}}$.

[c] Value in Columns 2-5 divided by value in Column 6.

[d] Value of preference equation for a strong Republican with mean value for other variables.

[e] Value of preference equation for a strong Democrat with mean value for other variables.

with no opinions). The exception is the larger negative constant term for the *agree* response in 1964. Given that the *agree* response indicates opposition to federal school integration actions, the negative coefficient indicates a reduced likelihood of an *agree* response for all preference levels, suggesting a general pro-civil rights shift among the mass public. Second, the coefficients relating the *indifferent* and *no opinion* response to preferences are close to .5, particularly for indifference. This suggests that both these categories function as an intermediate option between *agree* (where $A_1 = 1$) and *disagree* (where $A_1 = 0$) and constitute an ordinal scale with respect to preferences. This interpretation will be more apparent in the following discussion.

Unfortunately, coefficients in multinomial logit models do not lend themselves to easy interpretations, as the log-odds concept is not particularly intuitive. To assist in interpreting these coefficients, we walk through the model to show how the information in Tables 2 and 3 relate to the expected responses to the school integration questions. Table 4 shows the relationship between different values of 1960 school integration preferences (determined by the coefficients in column 3 of Table 2) and the probability of each response category (determined by the coefficients in Table 3). Columns 2–5 in Table 4 correspond to the numerator in equation 7 for each category and column 6, labeled SUM, corresponds to the denominator in this equation. The probability of a given response for a particular value of Y_1, shown in columns 7–10, is merely the ratio of the entry in the appropriate column to this denominator. Entries in Table 4 correspond to values of preferences at the mean (0.0) and at one, two and three sample standard deviations above and below the mean. Also included are the values of Y_1 for a strong Democrat (.53) and a strong Republican (−.73) with mean values for all other variables in Table 2.[9] We will continually refer to such individuals as our "otherwise average strong Democrat and strong Republican."

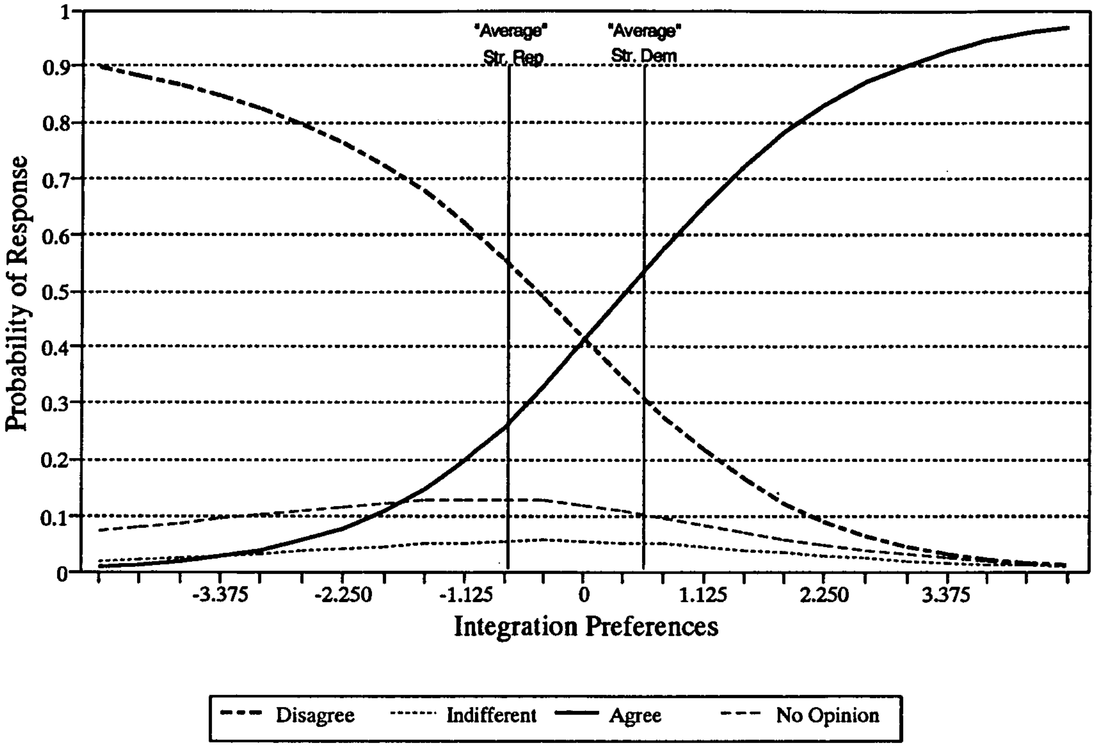
The first thing to note is that the probability of an *agree* or a *disagree* response changes dramatically with changes in the underlying preference variable. An individual with the mean preference has virtually equal probabilities (.41 and .42 respectively) of agreeing or disagreeing with the question. Just one standard deviation difference in preferences in either direction changes these probabilities by between .2 and .25. This indicates that differences in the value of Y_1 associated with the explanatory variables in Table 2 give large differences in likely responses. The peaking of the *indifferent* and *no opinion* response probabilities about the middle of the preference distribution illustrates the earlier observation that these responses function as a middle category for individuals with preferences at the center of the distribution, as one would expect from an ordinal scale.

The relationship between preferences and the probability of each response can be shown graphically, as we do in Figure 1. This figure plots these probabilities for values of Y_1 corresponding to the range shown in Table 4. The steep slope of the *disagree* and *agree* response curves illustrates the strong relationship between the estimated model in Table 2 and the likely response.

Using the information in Table 4 and Figure 1, we can illustrate how factors related to preferences shown in Table 2, especially partisanship, translate into differences in expected responses to the integration questions. This is done by relating preferences to expected responses for an otherwise average strong Democrat and strong Republican and comparing these expected responses. The expected value of $Y_{1,60}$ is .53 for a strong Democrat and −.73 for a strong Republican, giving the difference of 1.26 described previously. Table 4 gives the probability of each response for individuals with these preferences. For example, the Republican has probabilities of .55 of

FIGURE 1

Response Probabilities, School Integration, 1960



disagreeing with and of .26 of agreeing with the question about school integration. The comparable probabilities for the Democrat are .32 and .53. The vertical lines in Figure 1 picture these respective probabilities.

This analysis can be extended to other years to illustrate the expected differences in responses between otherwise average respondents who differ only in their party identification. Figure 2 shows the probability of each response for three such average respondents—a strong Democrat, a reported Independent, and a strong Republican—in 1956, 1960 and 1964. (We have transformed the responses so that in this figure *Favor* indicates the response favoring federal school integration efforts and *Oppose* indicates the opposite response.) For 1960, the bars correspond to the height of the curves in Figure 1 for the strong Republican, strong Democrat, and Independent. This figure, in effect, converts the coefficients on party identification, reported in Table 2, into response probabilities for different partisans. As indicated by the very small partisan coefficient in 1956, there are virtually no differences among partisans in expected responses that year. The already-discussed shift to greater support among Republicans in 1960 is quite apparent in the second set of bars. The reverse process occurred between 1960 and 1964, with Democrats becoming more likely than otherwise similar Republicans to support school integration policies, as depicted by the last set of bars.

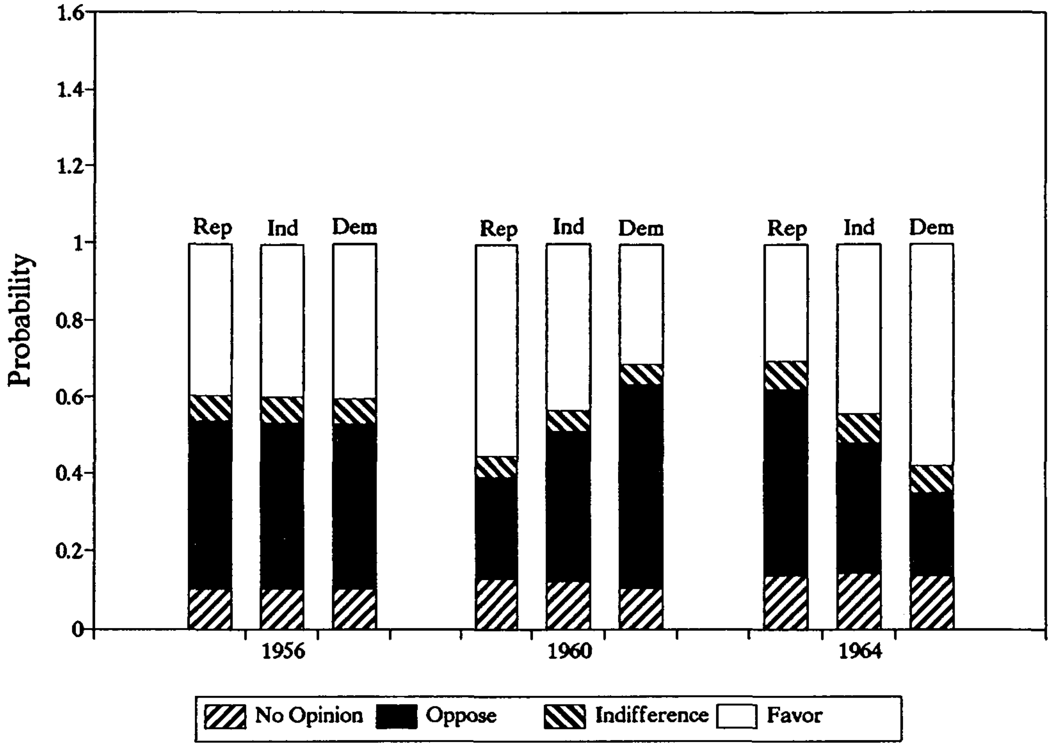
Equal Opportunity

The estimated preference models for the issue of equal access to jobs and housing are shown in Table 5. We will not go into as much detail about this model and the expected responses as we did for school integration but will simply highlight the important results. (Relating the differences in preferences shown in Table 5 to the different probabilities of favorable and unfavorable responses proceeds just as with the school integration issue.) The coefficients on party identification show the same pattern as on the school integration question, except that the partisan differences start out larger and change more. In 1956, Democrats are more likely to favor federal actions to insure equal access to jobs and housing than otherwise identical Republicans. Between 1956 and 1960 Democrats became more likely to oppose and Republicans to support these federal actions. Between 1960 and 1964 just the opposite movements occur, as we expected.

The strongest relationships between the exogenous variables and 1956 attitudes were the opposition of both southerners and those living in areas with a high proportion of blacks to, and the support of blacks for, federal efforts to ensure equal treatment. Support for these actions increased with age, education, and income, though the estimated coefficients are unreliable as indicated by their estimated asymptotic standard errors. The most likely real preference changes

FIGURE 2

Expected Integration Attitudes By Party and Year for Mean Respondent



between 1956 and 1960 are the increased support among Catholics and Jews relative to Protestants and the increased opposition of people living in predominantly black areas. This latter change appears to be transitory, however, as the change between 1960 and 1964 was of an equal magnitude but in the opposite direction. Otherwise, there were relatively few changes between 1960 and 1964.

The equations relating responses on the equal access questions to preferences for an average respondent are shown in Table 6. (The same normalization and scaling rules discussed with Table 3 are used here.) The large positive constant terms for the *agree* response in 1956 and 1960 reflect the earlier observation that people were expressing a considerable amount of support for this policy at a time when it was not yet being vigorously debated. The changes in the constant terms between 1960 and 1964 reflect the change in the overall support for this policy, with the average respondent being more likely to disagree with the policy in 1964 than previously. The coefficients showing the relationship between preferences and the probability of each response are similar to those observed for school integration and indicate a very strong relationship between the underlying preference variable and the likelihood of each response.

The results in Tables 5 and 6 permit comparisons of the expected responses of different partisans. Figure 3 shows the expected probability of each response to the equality question for three otherwise average respondents: a strong Democrat, an Independent, and a strong Republican. The high proportion in favor of federal equality efforts in 1956 and 1960 and the reduction in that proportion in 1964 is evident. Also evident is the same pattern of change among different partisans that we saw for school integration. Republicans, relative to Democrats, became more supportive of federal civil rights actions in 1960 and Democrats became more favorable between 1960 and 1964, corresponding to the shifts in party positions. On both of the civil rights questions, we have seen the pattern of preference change predicted by the endogenous preference model in equation 6.

Vietnam

Estimated coefficients for the Vietnam preference model are shown in Table 7. The expected association between partisanship and preference change is clearly evident. In 1968, at the end of the Johnson administration, Democrats were more likely than Republicans to want a more vigorous approach to the war. Between 1968 and 1970, there was a small relationship between partisanship and preference change, with Democrats becoming more dovish and Republicans more hawkish. Between 1970 and 1972, as the Democrats moved to a very strong antiwar position and Nixon dramatically escalated the war, the association between partisanship and preference change is even stronger. Democrats became even more dovish while Republicans became more hawk-

TABLE 5

Attitudes on Federal Guarantees of Equal Access to Jobs and Housing

VARIABLE	COEFFICIENTS IN PREFERENCE EQUATION, $Y_{1,t}$ 1956	1960	1964
Party ID[a]	.27 (.26)	−.73 (.28)	.92 (.22)
Male	.42 (.79)	−.43 (.18)	−.09 (.35)
Black	5.22 (2.34)	−1.46 (.67)	−.01 (.31)
Age/100	1.27 (1.76)	−1.68 (1.21)	−.08 (1.41)
Educ/10	1.15 (1.56)	−2.41 (1.17)	1.09 (1.26)
log(Inc)	.34 (.68)	−.46 (.34)	.16 (.42)
South	−.49 (.25)	.37 (.25)	−.20 (.17)
Jewish	.45 (.52)	.50 (.56)	.09 (.46)
Catholic	−.02 (.23)	.52 (.27)	−.12 (.17)
% black	−.1.41 (.99)	−2.12 (1.02)	2.16 (.64)
% urban	−.47 (.35)	.63 (.35)	−.01 (.25)
$Y_{i,t-1}$	—[b]	1	1
σ[c]	1.331	1.155	1.492

Note: Asymptotic standard errors are in parentheses.
[a] Instrumental variable for Party ID.
[b] No lagged preferences included.
[c] Standard deviation in preferences.

TABLE 6

Response Model for Federal Guarantees of Equal Access to Jobs and Housing

CATEGORY	VARIABLE	COEFFICIENTS IN RESPONSE MODEL 1956	1960	1964
No Opinion				
	Constant	−.27	−.21	−.91
	Preference	.29 (.33)	.37 (.15)	.28 (.09)
Agree				
	Constant	1.56	1.68	.54
	Preference	1	1	1
Indifferent				
	Constant	−.84	−.71	−1.40
	Preference	.84 (.18)	.44 (.19)	.59 (.10)
Disagree				
	Constant	0	0	0
	Preference	0	0	0

Note: Asymptotic standard errors are in parentheses.

ish. The association between party identification and preference change is far more prominent if we examine the change over the four-year interval 1968–72, where the implicit coefficient on party identification is .85.

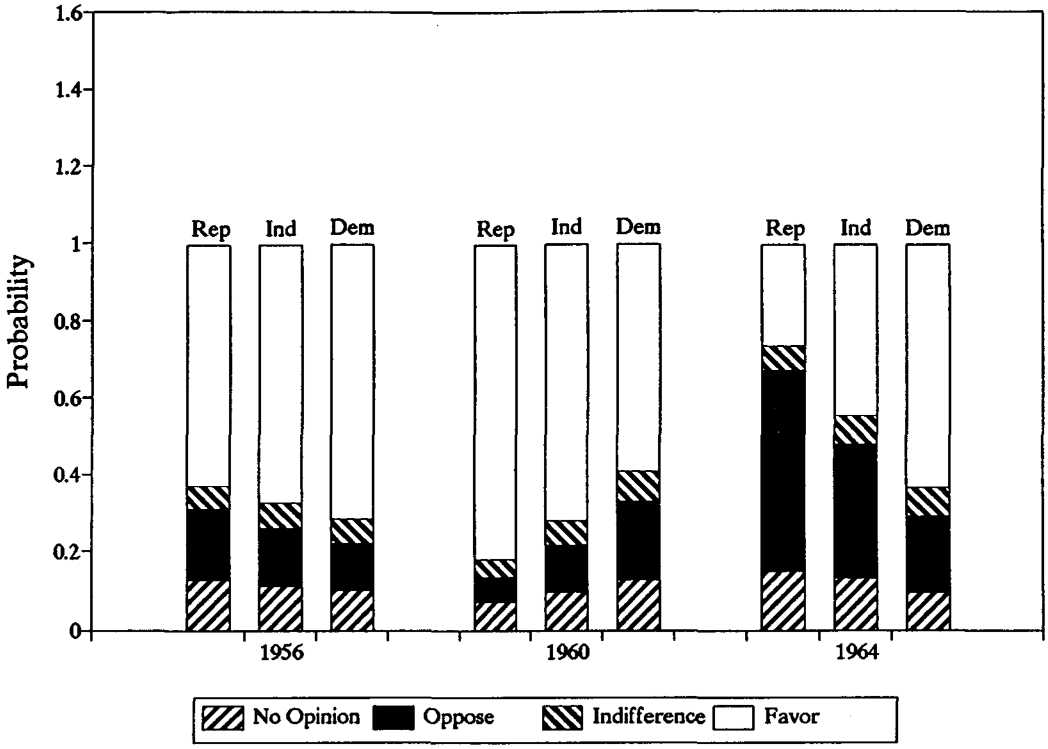
One of the most interesting relationships between the exogenous variables and preferences on Vietnam policy was the evolving relationship between attitudes on the one hand and age and education on the other. Other studies have explored the relationship between age, education, and Vietnam War policy preferences. Lunch and Sperlich (1979) find a strong negative relationship between age and support for the war, implying that older Americans were more opposed to U.S. involvement in Vietnam than were younger Americans. They find this relationship to hold throughout the entire Vietnam War era and offer several explanations for their counterintuitive findings. Even given their creative explanations, however, we should be skeptical of their results because they fail to control for other factors and they omit the possibility of an important age–education interaction. Mueller (1973) finds greater support among the youngest groups, but his bivariate analysis is also suspect. Verba and his colleagues (1967) find little or no direct relationship between education and Vietnam policy preferences. They, too, fail to control for other factors and omit an age–education interaction.

We find an important interaction between age, education, and preferences. In 1968, there is only a small association between preferences and education. Among 20-year-olds, the expected value of the preference equation is only .06 more hawkish for someone with 16 years of schooling than for someone with 10 years of schooling. Among 60-year-olds, the magnitude of the difference is only .02 in the opposite direction. By contrast, at every education level, there are large attitude differences associated with age (on the order of .55 to .6), with older respondents being more dovish.

These relationships changed dramatically by 1970. In 1970 and 1972, there is a very strong association between preferences and education, with better-educated respondents being more dovish, regardless of age. The association between preferences and age also changed, so that older respondents were more hawkish than younger ones for all education levels. Further, the age–education interaction was quite large and implies that the young *and* highly educated individuals were by far the most dovish. In 1970, the Vietnam preference index differs by 1.55 for a 20-year-old college graduate compared to a 20-year-old with only 10 years of schooling. Among 60-year-olds, the corresponding difference is only .67. Alternatively, the difference in the preference index between a 20-year-old and 60-year-old is only .37 if both have 10 years of schooling but is 1.25 if both have a college degree. These relationships—and the changes over the four years—fit our expectations except that one might have expected the changes to have occurred before 1968, earlier than appears here. The other

FIGURE 3

Expected Equality Attitudes By Party and Year for Mean Respondent



coefficients indicate that in 1968 support for the war was higher among males, nonblacks, southerners, Protestants, and union members.

The relationships of preferences to gender and race diminished somewhat between 1968 and 1972, while relationships of preferences to *south* and *being Jewish* increased. The association between preferences and union membership is particularly interesting, given the support union leaders expressed for the war. Not many can forget the image of "hard hats" confronting student protesters. The union variable's coefficient offers an important comparison with the coefficient on partisanship in the Vietnam preference model. We found the party coefficient to indicate that Democrats became more dovish and the Republicans more hawkish between 1968 and 1972. In contrast, union members remained more hawkish than nonunion members throughout this period. If the cues offered by union leaders influenced the preferences of the membership, this is the pattern of coefficients we would expect.

The relationship between preferences and the response categories each year are shown in Table 8 for a respondent with the average likelihood of not having an opinion. Again, there are very strong relationships between the value of the preference equations in Table 7 and the probability of each response. There is a noticeable dovish shift in the average respondent between 1968 and 1970 as evidenced by the constant term on the withdrawal and status quo options, indicating an increased probability of those responses.

Lastly, we illustrate the relationship between partisanship and Vietnam War preferences in Figure 4. This figure shows the expected responses of different partisans who are otherwise average, as defined in the previous figures. In 1968, Republicans are more likely than Democrats to prefer withdrawal and are less likely to want to seek victory. The distribution of responses changes over time, as Democrats become more likely to favor withdrawal and to oppose seeking victory. These proportions favoring different options shift dramatically among different partisans, as predicted by our model.

IMPLICATIONS

Two views of institutions have become popular in the social science literature.[10] One view sees institutions as the means by which individual preferences and actions are aggregated into collective outcomes (see Ostrom 1986; Riker 1980). This view of institutions is captured in how we model the competitive economic market and the spatial model of elections. Changes in the institutional structure (e.g., a move to monopolistic markets or to a multiparty system) imply subsequent changes in the social outcomes and possibly the loss of a unique equilibrium solution. The assumption of fixed preferences is central to analyses in

TABLE 7

Attitudes on Vietnam War Policy

VARIABLE	COEFFICIENTS IN PREFERENCE EQUATION $Y_{1,t}$ 1968	1970	1972
Party ID[a]	−.43	.29	.57
	(.30)	(.17)	(.31)
Educ/10	−.16	3.28	−1.03
	(.78)	(.78)	(1.18)
Age/100	1.06	2.54	.74
	(1.69)	(1.42)	(1.97)
Age * Ed/1000	.26	−4.30	−.31
	(1.38)	(.91)	(1.68)
Male	−.64	.30	.22
	(.15)	(.14)	(.24)
Black	1.46	.27	−1.05
	(.28)	(.22)	(.32)
South	−.30	−.11	.17
	(.17)	(.10)	(.22)
Jewish	1.33	.09	.37
	(.53)	(.34)	(.67)
Catholic	.36	−.34	.05
	(.19)	(.12)	(.21)
Union	−.19	−.06	.00
	(.19)	(.19)	(.24)
log(Inc)	−.05	.02	−.03
	(.13)	(.14)	(.13)
$Y_{i,t-1}$	—[b]	1	1
σ[c]	.660	.812	.765

Note: Asympototic standard errors are in parentheses.
[a] Instrumental variable for Party ID.
[b] No lagged preferences included.
[c] Standard deviation in preferences.

this vein. A second and quite different approach to institutions suggests that social processes and outcomes, translated through institutions, affect individual attitudes and actions (see March and Olsen 1984). Institutions give meaning to society and shape the way people develop their preferences and think and act upon them. As institutions function or when institutions are changed, individuals receive different stimuli and perceive a different world, characterized by altered patterns and relationships and leading to changes in values and preferences. Chamberlin (1990) presents a discussion of rules that define institutions and their aggregation properties in which he recognizes that institutions also affect individual behavior. Our results suggest the importance of exploring these complementary roles and how they might be synthesized. We now consider the implications of our findings and of such a synthesis for positive and normative studies of institutions. The problem for positive analyses concerns the modeling and estimation problems presented by situations where preferences are endogenous. The problem for normative studies concerns a necessary shift in how we evaluate and compare institutional performance.

TABLE 8

Response Model for Vietnam War Policy

CATEGORY VARIABLE	1968	1970 FORM 1	1970 FORM 2	1972
No Opinion				
Constant	−1.20	−.71	−2.31	−.64
Preference	.54	.49	.30	.43
	(.17)	(.13)	(.30)	(.19)
Withdraw				
Item 7				
Constant	−.46	.49	.34	.46
Preference	1	1	1.41	1
			(.21)	
Item 6				
Constant	—	—	−.67	−.41
Preference	—	—	1.46	.92
			(.24)	(.14)
Item 5				
Constant	—	—	−.35	.02
Preference	—	—	1.09	.61
			(.21)	(.14)
Indifferent				
Item 4				
Constant	.15	.48	.43	.66
Preference	.73	.68	.73	.20
	(.11)	(.08)	(.17)	(.17)
Item 3				
Constant	—	—	−.82	−.28
Preference	—	—	.27	.15
			(.22)	(.21)
Item 2				
Constant	—	—	−.96	−.86
Preference	—	—	.68	.03
			(.22)	(.26)
Victory				
Item 1				
Constant	0	0	0	0
Preference	0	0	0	0

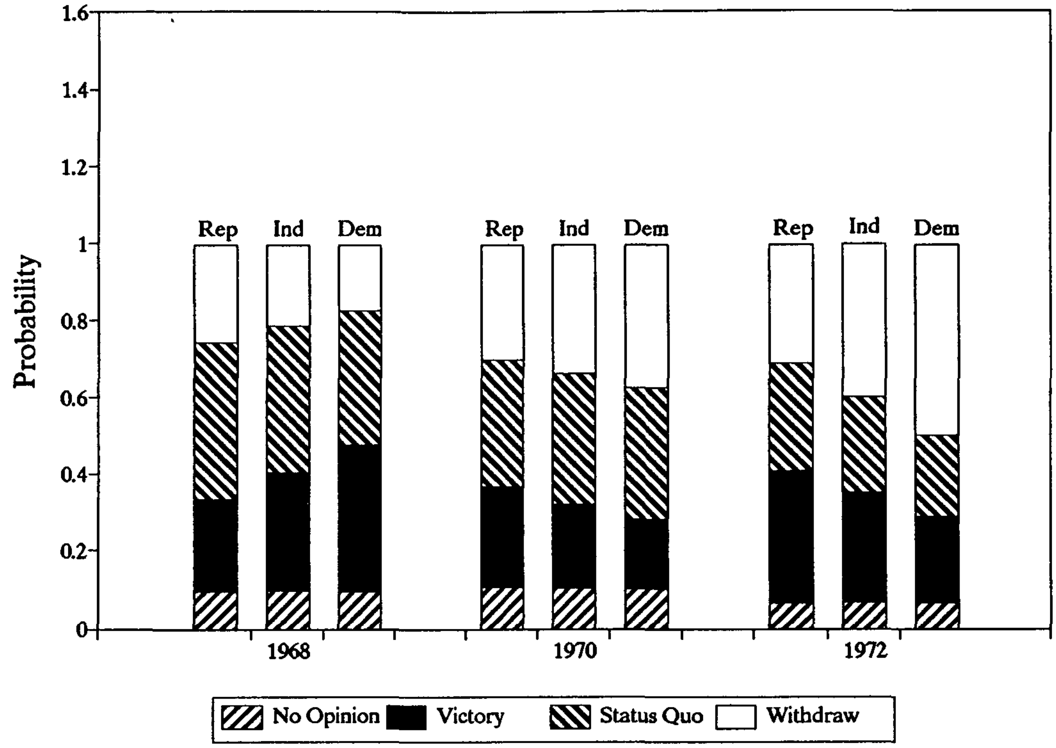
Note: Asymptotic standard errors are in parentheses.

Modeling Elections and Institutions

Endogenous preferences introduce a dynamic element into the traditional static electoral model. In static models with a unique equilibrium, such as those developed from the traditional spatial electoral model with a single issue dimension, outcomes are determined by the set of initial, or exogenous, conditions. Given enough elections, the search for a winning platform will ultimately lead to convergence to the mean of the distribution-of-preferences as the equilibrium result, regardless of how poorly informed the parties may be. With more than one issue dimension, this equilibrium result is more problematic and depends upon having a particular configuration of preferences and marginal utilities (see Jackson 1973; Plott 1967). Nevertheless, the outcome, even if it includes cycles, is solely a function of preferences and is independent of the sequence of platforms.

FIGURE 4

Expected Vietnam Attitudes By Party and Year for Mean Respondent



With endogenous preferences, as introduced to this model through equation 5, we have a substantially different model and set of possible results.[11] Specifically, outcomes become less predictable.[12] An equilibrium may exist under a variety of initial conditions, but any result will be partially dependent upon the sequence and timing of party platforms and upon how rapidly the parties change positions. For example, if both parties adopt platforms close to the mean, an equilibrium is reached quickly and there is little change in the distribution of preferences. Similarly, if initial party positions, partisanship, and subsequent changes in party positions are symmetrically distributed, meaning that the cues to one set of voters are completely offset by the cues to a second set, then there is both an equilibrium and a stable mean preference. By symmetric strategies we mean $\phi = -\theta$ and $(\theta_t - \theta_{t-1}) - (\phi_t - \phi_{t-1}) = 0$.

The critical dynamic is introduced when any of the symmetry conditions in the previous example change. The simplest extension is when one party, say Phi, adopts a new position, either purposely or stochastically, that is not symmetric with Theta's position. This platform will lead some individuals to adjust their partisanship and shift their preferences by some amount. The further Phi is from the symmetric position, the larger these shifts will be. Even if the parties converge on the new mean at the next election and this mean is stable, the parties have altered society's preferences and the political outcome. If the parties continue their pattern of nonsymmetric and nonconvergent strategies, the distributions of partisanship and preferences will continue to shift. The sequence of shifts and the ultimate outcome will depend upon the sequence of platforms. Different sequences of party strategies can produce substantially different preference distributions, such as are obtained by altering one of the party's initial platforms or by varying the rate at which one party adjusts its platform from one election to the next.

Under the dynamic model, parties have incentives to adopt nonmean positions. Once we allow party actions to change preferences, activists have a motive to gain control of the party platform, both to express their own policy preferences and to try to shift voters' preferences. This suggests that parties need to be seen as organizations that value more than just winning elections. Leaders may have more complex objective functions that include the positions taken by the party and the preferences of the electorate in addition to votes. (For a theoretical argument, see Aldrich 1983; Wittman 1983. For empirical evidence on these points, see McCloskey, Hoffman, and O'Hara 1960; Miller and Jennings 1986; Kessel 1988.) Thus some potential party leaders are likely to promote policies designed to do more than win elections. This incentive may lead to the asymmetric strategies that lead to even greater preference shifts and to outcomes that are even less predictable on the basis of an initial preference distribution.

The possibility of endogenous preferences presents a daunting agenda for scholars concerned more generally with studying social institutions. The difficulty is that new models that recognize the inherent dynamics of the institutional process are required to meet this challenge. The most useful models are likely to be ones that stress the dynamic aspects of social processes and that recognize their path dependency. It is unlikely that we can count on models that assume that a specified set of exogenously given conditions will lead to the same expected outcomes, with differences being merely the result of the addition of a stochastic term whose expected value is zero. Arthur, Ermoliev, and Roniovski (1987) present some very simple stochastic models in which there is a stable equilibrium but where chance events early in the sequence of actions determine the equilibrium. They also present several social and economic processes that seem to fit the conditions of this simple model. Exploration of such models and their implications for empirical work need to be pursued.

Evaluating Institutions

The normative issues raised by endogenous preferences are larger and less tractable. Much of the normative literature on institutions bases evaluations on how outcomes compare to the distribution of preferences. The dominance of concerns about Pareto optimality testify to the importance of this baseline. Chamberlin (1990) presents an excellent discussion of various criteria by which we evaluate institutions, and all are based on individual preferences and behavior. March and Olsen (1984) insist that a very important function for institutions is their role in helping individuals learn preferences and roles. From this vantage point, there must be a discussion and comparison of what preferences and roles are learned and how institutions affect this process.

All preferences may not be equally legitimate, even if we restrict our discussion only to those types conventionally considered. There is great debate in the political science literature about the existence and important of self-interests relative to collective interests (Hawthorne and Jackson 1987; Kinder and Kiewiet 1979a, 1979b; Sears et al., 1979, 1980; Wilson and Banfield 1964). If the relative salience of these two motivations is related to how arguments are framed, to the actions of political leaders and organizations, or to the context within which decisions are made, then discussions of the nature of preferences and how different institutions shape them become very important.

In many issues of major concern for public policy, preferences may be labile, uncertain, and poorly informed. In these instances, the learning of preferences may be as important as, or more important than, their aggregation. (For a model that stresses the evolution of preferences, see Cohen and Axelrod 1984.) Most traditional collective choice models assume that individuals have stable and rational preferences. Such assumptions seem appropriate in certain settings. Consider, for example, the traditional consumer choice paradigm, where one has often-repeated decisions, low information costs, and small costs for a mistake, such as whether to buy Bartlett pears or Cling peaches, each at a given price. In contrast, decisions with complex, long-term, and collective consequences that are made only infrequently may lead to situations with great uncertainty and poorly formed preferences. Consider such complex public policy decisions as whether to construct nuclear or conventional electric power plants, adopt a voucher system for funding education, or deregulate an entire industry. Studies of how people make choices among alternatives with very uncertain outcomes or low probabilities of very consequential outcomes stress the nonrational and at times very "plastic" nature of the preferences underlying these decisions. In these very important cases, the process of discussion and preference formation must precede preference revelation and aggregation. The role of institutions in developing preferences may be more important than their role as aggregator of these preferences.

Ultimately, one may face a choice between institutional structures with desirable aggregation properties, such as systems that equilibrate to the mean voter, and systems with desirable preference formation properties. This is a quandary that empirical institutionalists as well as theorists should relish. We must sharpen our conceptual and empirical models as well as our normative judgment in order to debate the question productively.

Notes

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1. Empirical research suggests that policy preferences are not fixed between—or even within—election periods. A considerable amount of effort in political psychology addresses this process (see Zaller 1991a). Analytically, however, most theoretical models of electoral processes treat preferences as fixed in order to study the properties of different electoral systems. This is the treatment we are characterizing at this point.

2. This argument distinguishes between adaptive utility models and models of incomplete information in which people are uncertain about a policy's likely consequences. In these latter models, information provided by political actors leads people to revise their beliefs about expected consequences and thus, possibly, to alter their behavior. See Calvert 1985; Gerber and Lupia 1992; Lupia 1992; McKelvey and Ordeshook 1985, 1986.

3. The results in Jackson 1992a question the strict quadratic form but are consistent with the traditional spatial model. The strict quadratic form is used here because of its traditional role and its ease of exposition and manipulation.

4. Throughout this development, zero denotes pure preference indifference and pure independence. Larger positive values for v_i denote an increasing preference for Theta, and larger negative values indicate an increasing preference for Phi. Analogous interpretations will apply to values of P_i.

5. Zaller (1991b) proposes a more elaborate model of opinion change in which the extent to which individuals accept or reject new information is mediated by their overall level of political awareness. This would imply an additional interaction term between partisanship and awareness in equation 5. In simplifying Zaller's model, which is necessary to accomodate subsequent estimation difficulties, we omit this interaction term. The likely consequence of this simplification is to understate the relationship between partisanship and preference change in equation 5 for the more aware respondents. If preferences are exogenous, as implied by the traditional model, we should still see no relationship between party identification and changes in policy preferences in our estimated equation. Thus, our omission of the interaction term will not bias the results in favor of rejecting the model of exogenous or fixed preferences.

6. The relationship between partisanship and attitudes on domestic questions that were affected by the same format changes as the civil rights questions reveals little change (Jackson 1988).

7. The exceptions were school integration in 1960 and 1964 and equality in 1960. Since we here focus on how party identification relates to attitude change, we do not discuss the response bias part of the model. For a full presentation of this model and how variations in the likelihood of having an opinion are related to each response category, see Jackson 1992b.

8. The log-odds are defined as the natural log of the ratio of the probability of any two responses. If we let $P_{4i} = Prob(Z_i = Disagree)$ and $P_{2i} = Prob(Z_i = Agree)$, then the log-odds of an *agree* relative to a *disagree* response, denoted by $L_{42,i}$, equals $ln(P_{2i}/P_{4i})$. Our normalization of these log-odds relative to the base category of *disagree* means that $L_{42,i} = A_{12}Y_{1i} + A_{22}Y_{2i} + A_{32}$. Further, the coefficient A_{12} is set to 1, which scales the preference variable, Y_1, so that the log-odds of an *agree* relative to a *disagree* response is just the equation shown in Table 2.

9. These values are calculated in the following manner. The value of Y_1 when all of the variables except party identification are at their sample mean is $-.10$. This means that the value of Y_1 is $.53 = -.10 + .63 * (1)$ for a strong Democrat and $-.73 = -.10 + .63 * (-1)$ for a strong Republican. Calculations either using these values and equation 7 and the entries for 1960 in Table 3 or reading the probabilities associated with .53 and $-.73$ from Figure 1 (shown by the vertical lines) give the probabilities of .53 and .26.

10. Following Jackson 1990, institutions are defined by specific rules for decision making, organizational designs, and behavioral norms and play an intermediary role linking individuals with collectivities.

11. Much of this discussion is based on a simulation model of elections with endogenous preferences developed in Jackson 1991.

12. For the rest of this discussion, we implicitly treat only the unidimensional case and assume that voting is stochastic and that parties are only interested in maximizing their votes.

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THE DESTRUCTION OF ISSUE MONOPOLIES IN CONGRESS

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Scholars studying congressional committees have noted the potential for members to seek membership on particular committees, leading to bias. Underpinning this line of scholarship is what might be termed a theory of comparative committee statics, characterized by a cross-sectional empirical approach. We present a new approach that focuses on the dynamics of jurisdictional control. By following a series of issues through the committee hearing process, we show that there is indeed significant issue bias in particular committee venues. However, we also find that new committees often claim jurisdiction over issues as they are redefined in the political process. The degree of jurisdictional monopoly enjoyed by different committees has been overlooked in the literature on this topic in spite of its importance in determining the nature of representation of interests in Congress.

Since Woodrow Wilson's classic study of Congress over a century ago, political scientists have developed a careful picture of the congressional committee system and its role in the American policy process (1956 [1885]). There is, of course, a great range in how committees in Congress operate, and the committee system as a whole has changed over time. Various reform movements have affected the balance of authority between committees and the central structure. Nevertheless, a reasonably clear and coherent theory of committee operation has emerged, with major formal and empirical developments in recent years. We might term this theory a theory of committee statics, because it treats the existing institutional structure of Congress as given. In formal approaches, this allows the analysis of committee activity under a set of parameters—basically, the rules governing the assignment of members to committees and the processing of bills. Similarly, the vast bulk of empirical studies of congressional committees has been cross-sectional in design.

In both formal and empirical research studies on congressional committees, one question has dominated: How do the views of committee members differ from the views of the chamber as a whole, and how might this potential bias affect legislative outcomes? A policy process lodged in policy subsystems centering on congressional committees could be quite different from one governed by the preferences of chamber members as a whole. Of course, no student of Congress ignores the great changes that have affected the institution over the decades; but our theories focus on the regularities in behavior, not the changes. Committees are "structure-induced equilibria" that both channel proposals and "empower veto groups" (Shepsle 1979, 36). That is, they are mechanisms for devolving policymaking to subsystems, yielding policy outcomes that can be different from those which might be supported by the chamber as a whole.

In this article, we first add important new empirical evidence that supports the static view of committee bias. Then we show that the standard static view is incomplete and probably misleading without an explicit consideration of the dynamics of jurisdictional control. Our new evidence on committee statics comes from considering the particulars of policy content as they are processed by committees. While most analysts of committee structures have focused on a given committee or set of committees, comparing the behavior or votes of their members to those of nonmembers, we adopt a different strategy. First, we identify a series of issues. Then, for each issue, we note the committees and subcommittees that have claimed jurisdiction over them at different periods in time. Our examination of the committee hearing process for a diverse set of issues, including pesticides, nuclear power, tobacco, and drug abuse, shows that committees tend to hold hearings that are favorable to the interests they oversee and that they attract primarily friendly witnesses. The results presented here are based on analysis of almost three thousand hearings during the postwar period.

In the second part, we consider committee dynamics. When we take a longitudinal view of the same issues that we use to show the policy-relevant cross-sectional biases, we find that these biases change dramatically over time and in ways that would not be predicted or understood by a cross-sectional view of the question. Committees are not granted permanent jurisdictional monopolies over the issues under their control at any given time. In fact, change in jurisdictional boundaries is quite common in Congress. Since the degree of jurisdictional control that any given committee may have over a particular issue may vary over time, the question of committee bias must be combined with a question of jurisdictional control in order to understand the impact of committees on policy outcomes.

As we shall see, the empirical findings from our analysis lead directly to the inference that the congressional committee system may be best depicted as a *network of punctuated equilibria,* with each committee possibly holding jurisdiction for a time, then losing out to other committees that are able to assert control over some aspects of a policy issue. When we add a consideration of the dynamics of jurisdictional control, we can see that a theory of committee statics cannot be used as a guide to understanding the policy role of the congressional committee system. Further, one can easily imagine that where committee behavior corresponds to the views of the parent chamber or falls within a zone of indifference or where deference to chamber norms seem more important than potential policy differences, few will complain or seek to alter the jurisdiction or power of the committee. Where controversy erupts over the perceived biases of committee members, any jurisdictional privileges that exist may be attacked and possibly eliminated. Controversy, issue definitions, and competition for control over emerging issues constitute important elements in the policymaking process within Congress; we pay special attention to these ideas here.

COMPARATIVE COMMITTEE STATICS

The term *comparative statics* has been used by Shepsle and Weingast to refer to equilibrium analysis that allows the comparison of outcomes under an assumed change of the underlying parameters (Krehbiel, Shepsle, and Weingast 1987). We use the term similarly, but in a broader context. In particular, we suggest a reconstruction of a diverse and voluminous literature that rests on three basic premises.

Proposition 1. *The committee system derives from the necessity of the division of labor within Congress.*

The committee structure allows the chamber to accomplish two goals. First, it allows specialization and what Herbert Simon has termed the parallel processing of issues (Simon 1983; see also Simon 1977, 1985). Second, as Shepsle (1979) has shown, it allows the chamber to overcome the problem of cyclical voting by limiting policy conflict to a single dimension within each committee. This allows the voting system within committees to reach equilibrium. "Jurisdictional arrangements not preference distributions create equilibrium" (Shepsle 1979, 48; see also Riker 1980, 1982).

Proposition 2. *Committee jurisdictions are based on policy content, even though other forms of division of labor are possible.*

That is, some committees in Congress handle procedural matters (e.g., the House Rules Committee), but most are policy specialists (Shepsle 1978). The decision to divide labor along policy lines has important implications, which we shall indicate.

Proposition 3. *The assignment of members of committees, while subject to institutional constraints, allows considerable room for the exercise of individual member objectives.*

In particular, subject-matter expertise and electoral considerations are important (Hall 1993; Hinckley 1971, 1975; Masters 1961; Shepsle 1978). As a consequence, committees are to a considerable extent formed from members who are interested in the committees' subject-matter jurisdictions. In recent years, the Senate has made committee membership more broadly available, to reflect a reward structure that has come to emphasize broad involvement in numerous issues, while the House has continued to offer fewer committee assignments and more policy specialization (Sinclair 1988); but this does not negate the central premise that policymaking within committees generally devolves to those interested in the policy area of question.

These three statements are not logical premises or assumptions but, rather, empirical findings. They find strong support in the literature and are not generally questioned by congressional scholars. From these basic premises, four somewhat more controversial propositions can be inferred and analyzed:

Proposition 4. *Committee recommendations carry strong weight in floor deliberations.*

The chamber is seen to give deference to committee positions because of institutional procedures, information advantages, and norms of reciprocity. Congressional scholars have engaged in spirited debate over the role of various procedures in promoting committee power (see Krehbiel, Shepsle, and Weingast 1987; Shepsle and Weingast 1987). Krehbiel and Rivers (1988) report that committees anticipate the majority chamber preference in reporting bills (but see Wilkerson 1991). Krehbiel has gone on to argue that committee power is not rooted so much in procedures as in the ability of committees to provide information to the chamber: "Committees earn the compliance of their parent chamber by convincing the chamber that what the committee wants is in the chamber's interest" (1991, 256). Although scholars have questioned whether committee power has waned under the reform rules of the 1970s, Krehbiel contends that this has not affected the fundamental manner in which committees operate. On the other hand, Smith and Deering (1990) indicate that the committee system is considerably weaker now than in the past, in part because of the flow of new issues that did not fall neatly into the existing jurisdictions of committees. This suggests that the relationship between the chamber and its committees may change over time (Sinclair 1989).

Proposition 5. *Congressional committees are the legislative linchpins of policy subsystems.*

Political scientists are not entirely agreed on the nature of policy subsystems and how enduring they are. The "ideal type" subsystem, described most elaborately by Maass (1951) for the Corps of Engineers in the late 1940s, involved a congressional committee, an executive agency, and one or more interest associations. Heclo (1978) and Berry (1989) have argued that the old pattern of tight-knit policy subsystems have given way to more fluid systems. On the other hand, Redford (1960, 1969) and Riley

(1990) suggest that there always existed substantial fluidity in many policy subsystems. At any rate, there is general consensus that policymaking is disproportionately influenced by interested parties and that congressional committees play the lead legislative role in acting as intermediaries between the chamber and the policy community, even where the policy community is fluid and sometimes conflictual.

PROPOSITION 6. *Committees' policy positions are often not representative of the chamber's, yet the chamber often defers to them.*

This proposition seems to flow so easily from the preceding that it was for a long time the prevailing wisdom—at least if one understands bias to apply to the policy arena and not to ideological orientation (see Niskanen 1971). The concentration of interested congressmen, the procedural advantage given to committee activities and the chamber norms of deference and reciprocity acted to give great advantage to interests organized through subsystems (Fenno 1973; Shepsle and Weingast 1987). In particular, the role of committees as agenda setters has been emphasized; that is, they can formulate propositions that can garner support in the general assembly and reject proposals that the majority might support but that are not in the interests of the constituencies of the members of the committee.

Surprisingly, however, Krehbiel (1990) reports that committee members are not generally preference outliers in their voting behavior in comparison to the entire chamber. And Krehbiel and Rivers (1988) suggest that even where committees are preference outliers, they may report bills closer to the median of the chamber in order to win support for their measures. Hall and Grofman (1990) have countered that it is not enough to compare committee members' roll-call votes with those of noncommittee members, since the latter may simply defer to the former's more intense interest in the issue. More persuasively, Hall and Wayman (1990) have argued that interest-group expenditures affect committee decision making in legislative markup sessions much more than in roll-call voting. Thus, they isolate a mechanism that would make committee proposals more responsive to interest-group influence than is member voting on the floor. Hall and his various coauthors have argued persuasively that committee membership affords members the opportunity not so much to vote differently from nonmembers as to become active in the development of legislation, affecting issues well before any floor action (Hall 1993; see also Hall and Grofman 1990). Looking only at roll-call votes could blind the researcher to important aspects of bias (see also Jackson and Kingdon 1992; VanDoren 1990, 1991).

PROPOSITION 7. *Because of the system of deference to the interested, the entire legislative process is biased.*

This is by far the most controversial proposition in the set. It is based on a chain of causation, namely, that congressmen get on committees because of their interests, that the distribution of preferences among the intensely interested is different from the distribution of preferences in the chamber at large, and that committee positions can prevail in chamber debates. It must reject the notion of counterbalancing—that, for example, liberals dominate the education and welfare committees while conservatives dominate agriculture. Davidson notes that committees may be biased in different directions, perhaps yielding little overall bias in the legislative process (1981, 111). Nevertheless, it survives as one of the folk wisdoms of Congress in operation.

TOWARD A THEORY OF COMMITTEE DYNAMICS

Political scientists who have studied congressional committees through cross-sectional comparisons have sometimes noted the importance of change over time. All students of congressional committees recognize the role of changes in institutional rules on outcomes both within committees and on the floor. Weingast, for example, has written that "the evolution from 18–22 stable committees to 125–50 more fluid subcommittees dramatically reduced the effectiveness of the previous mechanisms underpinning negotiating and maintaining bargains [to keep committee members from defecting in floor debates]" (1989, 812). What characterizes the cross-sectional approach most fundamentally, however, is that it supposes that committee performance is conditional on a preexisting set of parameters, the most important of which is the existing rule structure. Shepsle and Weingast comment: "A mature institution is, in our view, an organization in equilibrium. Changes in its environment ripple through the institution" (Krehbiel, Shepsle, and Weingast 1987, 943). The assumption of equilibrium implies that the basic parameters of policymaking—the institutional "givens" that structure the processing of issues—are unchanging (or change only according to some other well specified rules).

Some scholars have pointed toward a more dynamic picture of committee operation. Davidson and Oleszek (1977), for example, see the committee structure as jeopardizing reforms that tout more centralized control, because committees are subject to "clientelism." This suggests that the deference of the committees to their clienteles is in continual opposition to attempts by the legislative parties to impose central programmatic control. Such a dialectic is not easily reduced to equilibrium analysis, or at least, the models of committee equilibria that have been produced have not focused on this dynamic aspect of the relation between committee and chamber.

The most critical facet of committee behavior that is not compatible with the existing static view is how committee jurisdictions interact with developing policy issues. That is, committee statics treats issues as if they change little over time and as if they are easily and permanently assigned to the existing structure of committees. Shepsle and Weingast refer to committees (at least in the past) as "hegemons" (Krehbiel, Shepsle, and Weingast 1987, 942). Relaxing this unstated assumption brings into question the bias of individual committees and of the whole legislative system. If a committee is unable to maintain monopoly control over a policy issue, then membership bias of the committee is not particularly relevant.

Jurisdictional strife and competition among committees are important parts of congressional behavior (see King 1992). Committees are constantly changing their jurisdictions, both through attempts to grab parts of large issues as they become more important and through unavoidable redefinitions as new policy problems rise on the governmental agenda. In their study of energy politics in the 1970s, Charles Jones and Randall Strahan write that "congressional response [to the oil shocks of the 1970s] took on the character of the Oklahoma land rush. . . . The proliferation of committees and subcommittees claiming jurisdiction over energy policy dramatically expanded the number of members, congressional staff, and lobbyists participating in the issue area" (1985, 153; see also Jones 1975, 1979). It is important that Jones and Strahan attribute the change in committee jurisdiction to the expansion of the issue, rather than congressional reforms, though we should not ignore the possibility of their interaction. As issues hit the congressional agenda, jurisdictional structures change. Thus, congressional committee jurisdictions should never be assumed to be fixed; in fact, their definition is an integral part of the policy process (see Riker 1983, 1984, 1986).

Though congressional scholars have often focused on how committees and subcommittees with established jurisdictions protect themselves from encroachments from others, some scholarship shows that jurisdictional change may be common. First, bills are increasingly referred to more than one committee. These now constitute about 20% of all referrals and a substantially higher percentage of major legislation (Young and Cooper 1993, 213–15; see also Davidson 1989; Davidson, Oleszek, and Kephart 1988; and Schneider 1980). Second, while the parliamentarian must refer a bill to the committee that has established jurisdiction in that area according to precedent, "authors of bills have learned to draft them cleverly so that they can be referred to the 'right' committee" (Davidson and Oleszek 1977, 51). David King (1991) has shown how jurisdictional battles are often resolved not by reinforcement of the status quo but by the formation of new jurisdictions. This causes the continual realigning of committee jurisdictions in response to changing issue definitions.

When one considers the policy process generally, changes in decision-making structures are an important element of entrepreneurial strategies (see Riker 1986). As Schattschneider (1960) noted, losers in the policy process have an incentive to attempt to change the venue of decision making. Within Congress, this means that the choice of committee that might retain jurisdiction can be an important element in deciding the eventual outcome, so policymakers clearly have an incentive to attempt to manipulate it. Committee and subcommittee leaders similarly have an incentive to become active in areas where constituents may be affected, so there are both "push" and "pull" factors involved in jurisdictional dynamics. All of this does not mean that established jurisdictional boundaries can simply be ignored; of course they cannot. It simply means that there exist incentives for those within and outside of the committee structure to attempt to move issues from one body to another and that this potential dynamic may therefore be an important question to investigate.

Jurisdictional boundaries may be clearly delineated, and single committees may have exclusive jurisdiction over an issue. We term this state of affairs a *jurisdictional monopoly*. Jurisdictional monopolies seem especially common when an issue has low salience. In such policy environments, tight-knit policy subsystems thrive. This is the situation most often stylized as the structure-induced equilibrium in the formal literature. Committees that enjoy a jurisdictional monopoly and the general acquiescence of the larger body may find it necessary to hold few hearings, and those hearings that are conducted often have a "boosterism" quality to them. Such a situation is not guaranteed to be permanent, however. As Dion has pointed out, the equilibrium that might be induced by these structures will collapse quickly if the "previously indifferent" suddenly show greater interest and if their views are not the same as the interested committee members (1992, 476). This clearly is a variant of the expansion-of-conflict model described by Schattschneider decades ago.

As time goes on, issues that were once treated with indifference by the majority of legislators and relegated to the exclusive consideration of a single committee or group of committees may emerge on the public agenda. As the issue gains visibility, less deference to the wishes of a committee when a bill is considered on the floor is likely. Furthermore, other congressional committees may attempt to claim some jurisdiction through the scheduling of oversight hearings or the crafting of bills so that they will be steered away from traditional venues. These jurisdictional changes have the potential for much greater change in the long-run policy outcomes produced by Congress than the simple willingness or refusal of the floor to defer to the committee members. Through changes in jurisdictional assignments, old structures are replaced by new ones, thereby inducing new equilibria.

EMPIRICAL TESTS

The previous section argued for the importance of studying policy bias both cross-sectionally and longitudinally. In this section, we present empirical evidence on both these questions for several issues. First, we show the important cross-sectional bias that does indeed characterize much of congressional committee behavior. Second, we move beyond this question of bias by specialization to ask the question of the dynamics of jurisdictional monopoly. Committee bias is more important if it is coupled with jurisdictional control; where control must be shared with another set of committees that harbor different biases, then the overall effect may be mitigated. We shall therefore present first evidence concerning cross-sectional variation in committee behavior, then present the dynamic view.

Bias: Alive and Well in a Cross-sectional View

Studies of congressional committee bias have traditionally focused on the characteristics of members or their districts. The question of greatest interest has been whether committee members differ from the chamber at large in their policy preferences as revealed by either their voting behavior or the characteristics of the districts they represent (Hall and Grofman 1990; Krehbiel 1990; see also Hall 1992 and Hall and Evans 1990). Committees are more than assemblages of members, however. They are institutions with staffs, written and oral procedures, and networks of communications with executive agency personnel and lobbyists. They are venues for policy action. When viewed as an entire system, the structure of congressional committees and subcommittees affords many opportunities for strategic action. Committees, like other institutions of government, are dynamic structures. Fixed at any single time, they nonetheless change over the years.

In order to study the role of committees in influencing congressional agendas, we assembled and analyzed information on every congressional hearing concerning four important issues (civilian nuclear power, pesticides, smoking and tobacco, and drug abuse) either during the entire twentieth century or only during the postwar years—a total of 2,979 hearings. By noting systematically which committee and subcommittee held each hearing, we can trace which committees claim jurisdiction over which issues and note whether this jurisdiction is monopolistic or shared among competitors. For the cases of pesticides and smoking-and-tobacco hearings, we also noted information about each person who testified before Congress, noting (where possible) whether they represented industry, government, or lobbying groups and whether they were likely to be defending or attacking the industry in question. We gathered this information for over six thousand witnesses who testified on pesticides issues and over thirty-six hundred in the case of smoking. Because of the large scope and expense of that witness coding, we have limited this more extensive coding effort to only these two issues. While no selection of issues would be likely to constitute a truly representative sample of all possible issues, the data presented here constitute the largest effort to date to address these questions. They cover a range of types of issues and demonstrate a variety of properties concerning issue dynamics.

As venues for policy action, congressional committees and subcommittees may be more or less favorable to the major interests in a policy area. That is, while many committees may hold hearings on matters relevant to a given policy area, most committees can be categorized as likely to be more or less favorable to the affected interest. For example, agriculture committees are likely to be a more favorable venue for the pesticides industry than are health or environmental committees. While of course it may sometimes be difficult to reduce a policy debate to any single dimension, we find that for the issues chosen this simple dichotomy works remarkably well. In part, the ease in coding is due to our choice of issues. Since each of the issues discussed here includes an affected industry, we can typically break our coding down according to a simple rule. When coding the "tone" or the "venue" of a hearing, we asked, Would an industry leader be pleased or disappointed that these questions were being raised or that they were being raised by this group rather than by another?[1]

According to our simple coding rule, discussion of toxic pesticides residues on apples is coded "negative," because a pesticides industry official would probably prefer to discuss such "positives" as increased crop yields, decreased loss to pests, or improvements in safety techniques. If any of these issues were considered in the agriculture committees, then the venue would be coded as positive; if discussed in an environmental or health committee, then the venue would be coded as negative. By coding these two concepts—tone and venue—separately, we can empirically assess the differences in preoccupation of different sets of congressional bodies. In addition, our coding of witnesses appearing before these different bodies allows us further to investigate the potential bias of different groups of committees and subcommittees.

Our most important innovation in studying committee bias is in following a policy issue rather than a committee. That is, we pick an issue (e.g., pesticides policy) and then note which committees and subcommittees have held hearings on the topic. Thus, our method is (1) to isolate hearings on a topic, using Congressional Information Service keywords; (2) to note the bodies holding hearings; (3) to code the topics covered as generally favorable or hostile to the industry involved; (4) independently to code the probable favorableness of the committee venue toward the affected industry; and (5) in the cases of pesticides and smoking policy, to code witnesses as either favorable or unfavorable to the industry.

Committee Witnesses: Preaching to the Converted

For every hearing on the topic of pesticides from 1900 to 1988, we have coded each witness as representing a certain kind of interest: (1) agricultural, (2) environmental or health, or (3) other or uncodable. Agricultural interests included officials from the U.S. Department of Agriculture, farmers' organizations, and pesticides manufacturers, as well as the individual

TABLE 1

The Correspondence between Witness and Venues in Congressional Hearings on Pesticides, 1900–1988 (%)

VENUE OF HEARINGS	TYPE OF WITNESS: AGRICULTURE OR PESTICIDES INDUSTRY REPRESENTATIVE	HEALTH OR ENVIRONMENTAL REPRESENTATIVE	TOTAL (N)
Agriculture	75.7	24.3	100.0 (2,103)
Health or environmental	27.5	72.5	100.0 (3,128)
Other or uncodable	47.9	52.1	100.0 (746)
Total	47.0	53.0	100.0 (5,977)

Note: Gamma = .78; tau-*b* = .47; chi-squared (1d.f.) = 1,174.2 ($p < .001$). An additional 996 witnesses could not be coded; they appeared in omnibus appropriations hearings and discussed other issues.

members of Congress whom we could identify as having strong proagriculture views. Environmental or health witnesses included officials from environmental organizations, health officials, and others whose affiliations made clear that they were health or environmental experts, rather than farming experts. Table 1 shows that of a total of 6,973 witnesses who appeared in hearings during the century, we were able to classify 86% of them into one of these two substantive categories. Intercoder reliability for this coding of witnesses was 98.2%. (For another example of coding witnesses at congressional hearings, see Jenkins-Smith, St. Clair, and Woods 1991; for another use of hearings data, see Hansen 1991.)

In the case of smoking, similar coding was performed, distinguishing among witnesses representing agricultural interests (including cigarette manufacturers), trade questions (usually concerning foreign restrictions on the import of American cigarettes), health questions, taxes, and an uncodable category. Two complications arise in the case of smoking that lead to a large number of uncodable witnesses: appropriations and trade hearings often tend to be omnibus affairs in which smoking-and-tobacco questions are but one of many items on the agenda. In these cases, we have included the witnesses discussing smoking and tobacco but omitted all others. Thus, we are left with a total of 3,616 codable witnesses, as shown in Table 2.

We coded the committees and subcommittees holding the hearings in the same way as we coded the witnesses. Some committees were clearly proagriculture, others were clearly preoccupied with health and environmental questions, and still others could not be coded. In all, there were 385 hearings on pesticides topics from 1900 to 1988, held by 39 different committees and 89 different subcommittees. We have coded each of these committees or subcommittees as being part of the pro-agriculture venue, the health and environmental venue, or as part of another or uncodeable venue. This venue coding was done by referring to the title of the committee or subcommittee in question, independently of the topics of the particular hearings. Therefore we are confident that this venue coding is distinct from the question of tone of the topics considered, or the witnesses appearing before them. When this venue coding for pesticides hearings was done by a second coder, 95.2 percent of the cases were coded identically.

In the case of smoking, similar coding was performed, though Table 2 shows that we distinguished among three types of bodies: agriculture and trade bodies, which tend to promote the industry; health bodies, which tend to be more critical; and taxation bodies, which are negative, but in a different way than the health bodies.

Table 1 presents the relationship between the type of witness and the type of venue for pesticides; Table 2 shows the same relationship for smoking. Table 1

TABLE 2

The Correspondence between Witnesses and Venues in Congressional Hearings on Smoking, 1945–1986 (%)

VENUE OF HEARINGS	TYPE OF WITNESS: AGRICULTURE, TRADE, TOBACCO INDUSTRY	HEALTH	TAXATION	TOTAL (N)
Agriculture, trade	95.2	2.7	2.1	100.0 (2,510)
Health	28.3	71.4	.2	100.0 (829)
Taxation	21.7	17.0	61.4	100.0 (277)
Total	74.2	19.6	6.2	100.0 (3,616)

Note: Gamma = .91; tau-*b* = .71; chi-squared (4 d.f.) = 3,466 ($p < .001$). An additional 4,078 witnesses could not be coded. This large number stems mostly from appropriations and trade hearings, where smoking is one of many topics discussed; we coded only those witnesses discussing smoking.

TABLE 3

The Correspondence between Tone and Venue in Congressional Hearings on Pesticides and Civilian Nuclear Power (%)

	TONE			
VENUE	POSITIVE	NEUTRAL OR UNCODEABLE	CRITICAL	TOTAL (N)
Pesticides hearings, 1900–1988[a]				
Agriculture committees	17.6	3.9	78.5	100.0 (102)
Neutral or uncodable committees	11.2	6.3	82.5	100.0 (63)
Health or environmental committees	1.8	5.0	93.2	100.0 (220)
Totals	7.5	4.9	87.5	100.0 (385)
Civilian nuclear power, 1944–86[b]				
Booster committees	20.0	28.0	52.0	100.0 (611)
Neutral or uncodable committees	6.3	52.0	41.7	100.0 (352)
Critical committees	8.8	7.7	83.5	100.0 (274)
Total	13.6	30.3	56.1	100.0 (1,237)

[a] Gamma = .48; tau-*b* = .20; chi-squared (4 d.f.) = 26.9 ($p < .001$).
[b] Gamma = .30; tau-*b* = .18; chi-squared (4 d.f.) = 190.5 ($p < .001$).

shows that agricultural committees tend to schedule testimony from propesticides witnesses, while health and environmental committees are more likely to receive testimony from those critical of the industry. For the case of smoking, the relationship between venue and witness is even stronger, making it clear that committees like to hear from their allies, not their opponents.

When an agriculture committee has hearings on pesticides topics, its members are three times more likely to listen to testimony from representatives from the pesticides industry or from others likely to have a favorable view on pesticides than from health or environmental representatives. When a health committee holds hearings on a similar topic, the same selection bias in witnesses occurs: almost three-quarters of the witnesses appearing before these committees are health or environmental experts, rather than agricultural officials. We can also note from Table 1 how much more active the health and environmental committees have been in holding hearings on pesticide matters as compared to the agriculture committees. Over 3,100 people have testified before these committees on pesticide concerns, as compared to fewer than 2,300 before the agriculture committees. Of 385 hearings on pesticide topics coded, 220 have been in the health or environmental venue, as compared to only 102 in the agriculture venue.

The relationship between witness and venue in the case of smoking could hardly be more powerful. Agriculture and trade committees, when considering tobacco questions, simply do not schedule witnesses who could be expected to attack the industry. Congress specializes. When pesticides are to be attacked, one group of committees holds those hearings, and one group of members hears their testimony. When pesticides are to be defended, another committee holds those hearings and schedules a different group of experts to give testimony. Committees use hearings to garner support for the views they already hold (see also Del Sesto 1980).

Saying What They Want To Hear: The Venue–Tone Connection

As noted, we have coded each hearing on pesticides, smoking, drug abuse, and civilian nuclear power by the tone of its title and description in the Congressional Information Service abstracts. In the cases of pesticides, smoking, and nuclear power, we coded hearings simply as whether the title and abstract suggested that the tone was favorable or antagonistic toward the industry. For drug abuse, we coded hearings according to whether they emphasized an enforcement or suppression option, on the one hand, or an education or treatment option, on the other. Intercoder reliability for the tone-of-coverage coding was 96.0% for pesticides. We also coded the committees and subcommittees holding hearings in a manner similar to the one described for pesticides.

The results from this procedure are presented in three tables. Each classifies the venue within which the hearing was conducted as against the tone. Table 3 presents this tabulation for pesticides and civilian nuclear power; Table 4 contains information on drug abuse; Table 5, information on smoking and tobacco. For pesticides and nuclear power, most hearings are of a critical nature, regardless of the venue. For example, for pesticides, 78.5% of hearings before agriculture committees or subcommittees had a critical tone. Health and environmental committees were even more prone to scheduling hearings critical of the industry: 93.2% of the topics these committees and subcommittees examined were negative toward the industry. A similar pattern is evident for nuclear power: when hearings are held, they tend to be critical. In the case of nuclear power, the pronuclear

TABLE 4

Tone and Venue, Congressional Hearings on Drug Abuse, 1945–86 (%)

TYPE OF COMMITTEE	FOCUS OF ATTENTION: LAW ENFORCEMENT	FOCUS OF ATTENTION: EDUCATION, HEALTH	TOTAL (N)
Enforcement committees	74.7	25.3	100.0 (561)
Education, Health committees	15.5	84.5	100.0 (283)
Other or uncodable committees	64.5	35.5	100.0 (200)
Total	45.9	45.1	100.0 (1,044)

Note: Gamma = .88; tau-*b* = .56; chi-squared (1 d.f.) = 265.68 ($p < .001$)—statistics based on the four coded cells only. Since virtually all hearings share a negative tone toward the illicit drug industry, we code the hearings by their focus on law enforcement and interdiction matters or on education and treatment matters.

set of committees (esp. the defunct Joint Committee on Atomic Energy) have been considerably more positive in tone than the health and environmental committees. (Only 52% of their hearings were critical in tone, versus 83.5% of those held in the rival group.)

Even a committee generally favorable toward nuclear power must occasionally hold hearings on topics, such as radioactive leaks, that industry leaders would just as soon see discussed in private. Table 3 shows that much of congressional oversight through the hearings process focuses on problems in the industry in question. Even "favorable" venues are home to much critical oversight in these areas. Tables 1 and 2 give some indication that more favorable venues are not home to as much criticism as the critical venues, even when they hold hearings on the same topic. There we noted a strong bias in witness participation between the two venue classifications for pesticides and smoking policy. Our reading of abstracts, witness lists, and committee prints of hearings suggests that committees favorable to an industry tend to hold hearings that address an alleged problem; but their witnesses may be expected to dismiss it (see Del Sesto 1980). Hence, the subject of the hearing is not what the industry wants on the congressional agenda; but favorable committees are, at least, examining the problem from the industry's perspective. We shall see that efforts to contain conflict within favorable committees are not always successful. As the tone turns negative, attention tends to increase. As attention increases, it tends to spread. As it spreads, a jurisdictional monopoly supporting close and positive relations with an affected industry can be destroyed.

In the cases of drug abuse and smoking-and-tobacco hearings (Tables 4–5), there are more striking differences among committee venues regarding their tone. In the case of smoking and tobacco policy, 57% of hearings in agriculture or trade committees were on topics that we considered favorable toward the tobacco industry, while only 10.8% were unfavorable. For health and taxation committees (where cigarette taxes are considered), 73.1% of hearings are critical. Here, one gets the "ideal type" of venue bias: the protobacco venues almost never consider any topics that are unfavorable toward the industry. On the other hand, the unfavorable venues never consider anything that might be construed as favorable toward the industry.

For drugs, no hearings are favorable toward the industry, so we coded hearings according to the policy solutions to the drug problem. The data show vividly the disaggregated approach to an important policy problem, with each venue considering a set of solutions in isolation from others and with little reference to other options. Members of two separate solution-based subsystems seem to be talking only to themselves.

In general, this is the pattern that we have found for all of our issues. Examining the relationship between venue and tone reveals considerable support for the thesis that congressional committees are strongly biased. Proindustry committees do not often schedule critical hearings; and when they do, they

TABLE 5

Tone and Venue, Congressional Hearings on Smoking and Tobacco, 1945–86 (%)

TYPE OF COMMITTEE	TONE: FAVORABLE	TONE: NEUTRAL	TONE: CRITICAL	TOTAL (N)
Agriculture or trade	57.0	32.0	10.8	100.0 (203)
Health or taxation	2.9	24.0	73.1	100.0 (104)
Other	—	66.7	33.3	100.0 (6)
Total	38.1	30.0	31.9	100.0 (313)

Note: Gamma = .91; tau-*b* = .62; chi-squared (2 d.f. = 137.1) ($p < .001$)—statistics based on the six cells coded by type of committee only.

TABLE 6

The Relationships between the Number of Distinct Congressional Committees Holding Hearings on a Topic and the Total Number of Hearings in a Year, 1945–86

	EQUATION 1 (N = 42)				EQUATION 2 (N = 41)				
POLICY AREA	R^2	INTERCEPT	NUMBER OF HEARINGS	D–W	R^2	INTERCEPT	NUMBER OF HEARINGS	LAGGED NUMBER OF DISTINCT COMMITTEES	DURBIN'S H
Nuclear power	.88	1.74	.23*	1.57	.91	1.01	.15*	.38*	.496
		(.49)	(.01)			(.50)	(.03)	(.11)	
Drug abuse	.96	.85	.27*	2.12	.96	.79	.26*	.04	−.744
		(.33)	(.01)			(.35)	(.02)	(.09)	
Smoking	.84	.55	.57*	1.71	.84	.32	.53*	.12	.231
		(.34)	(.04)			(.39)	(.05)	(.08)	
Pesticides	.86	.92	.43*	1.11[a]	.92	.43	.29*	.39*	.627
		(.32)	(.03)			(.27)	(.03)	(.07)	

Note: The dependent variable for all equations is the number of distinct committees holding hearings on a given topic in a given year. Entries are unstandardized regression coefficients (standard errors in parentheses). Equation 1 estimates a model without lags: ($Y_t = a + b_1x_t + e_t$). Equation 2 estimates a model with a lagged endogenous variable: ($Y_t = a + b_1x_t + b_2y_{t-1} + e_t$).

[a]Test indicates potential autocorrelation. Equation 2 model shows no autocorrelation problem.

*p < 05, two-tailed test.

hear from proindustry representatives. Similarly, critical venues tend to schedule witnesses that provide unfriendly testimony. In sum, committees neither seek nor receive balanced, objective policy information and thus cannot be viewed simply as information-gathering agencies of Congress.

Our analyses of policy issues, rather than particular committees, shows that policy issues are often divided among many committees. More importantly, these committees can generally be grouped into supportive and critical venues; the committees specialize in defending or in attacking some policy. We can conclude that committees in Congress may well be biased in their members' policy preferences, but this fact alone does not allow us to infer that the entire system of committees is biased. Overall levels of bias depend on the degree of jurisdictional monopoly the committees are granted and on the dynamics of jurisdictional control among committees, a subject to which we turn next.

Attention and Jurisdictional Control

Jurisdictional dynamics imply that as hearings on an issue increase, the number of bodies conducting these hearings should also increase. Indeed, we have found that in each issue we have studied, a rise in attention is always accompanied by an increase in the number of distinct congressional committees claiming some part of the jurisdictional action. In the cases of nuclear power, pesticides, smoking and tobacco, and drug abuse, correlations between number of hearings per year and the number of different committees holding them are uniformly close to 1.0, as we shall see. Increased attention almost always leads to a breakup of a jurisdictional monopoly. Table 6 shows the close relation between the number of hearings and the number of different committees holding them. It reports the results of simple regressions in which the number of committees holding hearings in a given year is the dependent variable and the total number of hearings held is the independent variable:

$$Y_t = a + b_1X_t + e_t,$$

where Y_t is the number of committees holding hearings on the topic in a given year[2] and X_t is the total number of hearings held by all committees on that topic in a given year.

A rise in congressional attention tends systematically to be associated with an increase in the number of different congressional committees claiming jurisdiction; and the relationships are very strong. These powerful relationships make one suspect a tautology. Certainly it is possible that as Congress created more committees and subcommittees, these new bodies had to hold hearings on something. Thus, as the number of bodies increased, so did the number of hearings. However, the increases we note in our four issue areas do not occur only after the congressional reforms expanding committee and subcommittee power in the mid-1970s, but before then, as well. This would seem to indicate the operation of a conflict expansion model, rather than one of bureaucratic aggrandizement.

We may examine the issue of the direction of causality in more detail. The left-hand side of Table 6 presents ordinary least squares estimates, one of which involves potential autocorrelation problems. Lagging the dependent variable and including it explicitly in the equation is both a potential remedy for the statistical problem of autocorrelation and a way of controlling for the hypothesis that bureaucratic expansion (capacity to hold hearings) drives the number of hearings held. This is possible because now the number of committees holding hearings is, in effect, a change from the previous year's number of committees. So now we estimate the model as

$$Y_t = a + b_1X_t + b_2Y_{t-1} + e_t$$

Estimates are presented on the right-hand side of Table 6. The effect of hearings on bodies survives the lagged controls; the number of hearings is statistically significant in all equations in the table. The lagged variable is sometimes significant as well, indicating that the capacity to hold hearings is also important. The lagged variable technique also remedies the autocorrelation problem.[3] It appears that both issue expansion and bureaucratic aggrandizement contribute to the growth of Congress's capacity to hold hearings.

Conflict Expansion and Jurisdictional Dynamics

If the expansion of conflict in a policy subsystem is associated with a decline in the jurisdictional monopoly of congressional committees, then we can expect a decline in the proportion of hearings held within the dominant venue as the number of bodies holding hearings increases. That is, as conflict expands, committees and subcommittees not previously active on an issue hold hearings—and these new arenas tend to be unfriendly places for defenders of the status quo. Second, as the number of hearings increases, the proportion of hearings that have a favorable tone for the regulated industry or the existing policy subsystem should decline.

Several specific models are consistent with our general hypotheses, and we would not expect the process to follow the same pattern in every case. For example, in some areas, collapse in jurisdictional monopolies may occur very rapidly in the face of challenge, while in others there may be protracted conflict over control of the issue. In some cases, one jurisdictional monopoly may be replaced with another, while in another it may be permanently divided. Before we proceed to regression estimates of jurisdictional dynamics, it is worthwhile to pause briefly to consider some specific cases. Figure 1 illustrates the relation between attention and venue for the case of drug abuse.

During the period before attention to drug abuse was high, virtually all hearings on the topic of drug abuse were held within the confines of committees that generally focused on enforcement and interdiction options. As the number of different bodies (committees and subcommittees) jumped in the late 1960s and early 1970s, this jurisdictional monopoly was destroyed. From 1970 onward, no single group of committees or subcommittees could be said to control congressional attention to drug abuse questions: the proportion of the hearings held before enforcement-type committees was only slightly higher than that for the education and treatment committees. From a jurisdictional monopoly, drug abuse policy became subject to considerable jurisdictional wrangling. (It is worth noting that the expansion of the number of bodies holding hearings occurred prior to the congressional reforms of the mid-1970s. These policy expansions were probably institutionalized, rather than created, by the reforms; see King 1991.)

FIGURE 1

Monopoly Jurisdiction and Number of Congressional Bodies Holding Hearings: Drug Abuse, 1945–86

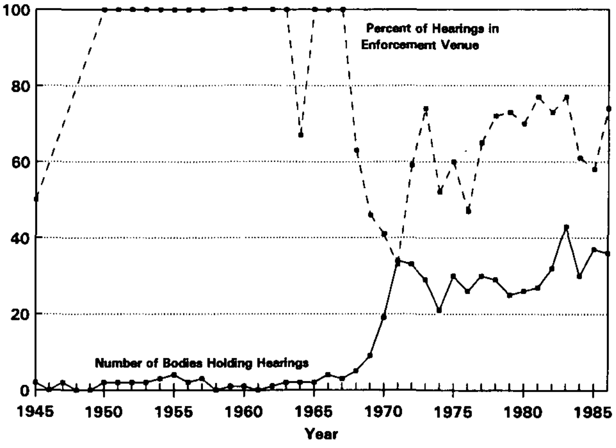
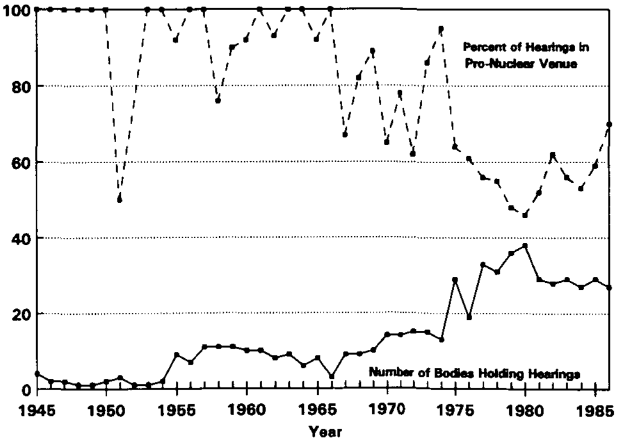


Figure 2 shows the case of civilian nuclear power. While the Joint Committee on Atomic Energy once held virtual monopoly power in this case, the pronuclear institutional structures within Congress were eventually eliminated, beginning in the 1960s, as the percentage of hearings within "favorable" venues began to decline. This process accelerated in the

FIGURE 2

Monopoly Jurisdiction and Number of Congressional Bodies Holding Hearings: Nuclear Power, 1945–86



1970s as the number of competing bodies rose and as the jurisdictional monopoly of the pronuclear committees was transformed into a veritable free-for-all, with both sides holding almost equal numbers of hearings.

As Figures 1 and 2 suggest, increases in the number of bodies claiming some jurisdiction over an issue lead to a decline in jurisdictional control for the group that once enjoyed monopolistic control. From Table 6 it is clear that we could have reported the total number of hearings rather than the number of different bodies holding them in Figures 1 and 2; the correlations are so high that either series would show a similar pattern. (The *lowest* correlation between the total number of bodies and the number of hearings held in a given year for the four cases is .95.) Our understanding of the process of conflict expansion indicates that it is not the additional hearing per se that leads to the breakup of a jurisdictional monopoly but the fact that these increases almost always stem from an incursion of new congressional bodies into areas that had previously not interested them.

We may now turn to more quantitative estimates of the process of jurisdictional dynamics. In order better to capture the competitive versus monopolistic nature of the jurisdictional situation, we transform the dependent variable by taking the absolute value of its deviation from 50. That is, a score of zero on the dependent variable would mean that half of the hearings are held in a hostile venue, half, in a favorable venue. A score of 50 means that all of the hearings are held in the same type of venue (be it favorable or hostile). By transforming the variable in this way, we make clear that many bodies claiming control should be related to great conflict (and so, a low score on the dependent variable); few bodies claiming control should be related to a monopolistic jurisdiction (and a score on the dependent variable close to 50).[4] If our expectations are correct, there should be a strong negative relation between the number of bodies holding hearings and jurisdictional control. In order to determine whether there may be diminishing marginal returns to continuing increases in the number of bodies holding hearings, we estimate a logarithmic decline model, as well as a standard linear model.

In Table 7 we estimate two models:

$$M_t = a + b_1Y_t + e_t$$

and

$$M_t = a + b_1 lnY_t + e_t,$$

where M_t is the monopoly control variable and Y_t is the number of bodies holding hearings.

Table 7 shows an impressive level of fit for our model of jurisdictional dynamics. The dependent variable in all equations is monopoly control, as described above; the independent variable is the number of bodies holding hearings on the issue. On the left-hand side of the table are standard ordinary least squares estimates, but two of the equations indicate potential problems with autocorrelation. In those cases, we have reestimated the equations including this lagged endogenous variable as an additional control.[5] Equation 2 in Table 7 estimates the following models:

$$M_t = a + b_1Y_t + b_2M_{t-1} + e_t$$

and

$$M_t = a + blnY_t + b_2M_{t-1} + e_t.$$

The right-hand side of the table presents the new estimates, and standard tests indicate that they are free of autocorrelation problems. In every case but one, the coefficients presented in Table 7 show statistically significant and substantively important results for our idea that jurisdictional incursions virtually always cause the breakup of whatever cozy arrangements might have existed between policymaking groups in Congress. Further, the logarithmic decline model of monopoly control shows significant results in every case, generally outperforming the linear model. This indicates that jurisdictional control may be more difficult to maintain in Congress than some have suspected, because increases in the number of distinct congressional masters have an especially important impact as that number increases from a very restricted set to a slightly larger set. Increases above a certain limit have relatively little additional impact. From the point of view of a former subsystem participant, the damage has already been done by time the number of congressional masters has increased from a very small number.

We showed in Table 6 that increases in hearings are related to increases in the number of different committees holding hearings. One might say that the demand for attention (hearings) affects the supply of opportunities to be heard. As public and congressional attention focuses on a new problem, this is rarely contained within a single set of committees; new committees are not shy about claiming some part of that jurisdictional action. This has important policy consequences, since Table 7 indicates that the expansion of hearing opportunities interferes with monopoly control of those issues. Though the results indicate that each issue does not follow precisely the same pattern, the general trend is ubiquitous: as more bodies become active, jurisdictional monopolies are destroyed. The better fit of the logarithmic model in each case shows that initial intrusions into new jurisdictions are especially important in destroying monopoly control.

It would seem that jurisdictional monopolies may be more fragile than many students of Congress have realized. Jurisdictional battles are a constant and important part of legislative politics. Further, there is considerable malleability in the domains covered by different bodies within Congress. As Jones and Strahan (1985) pointed out in the case of energy policy, when issues hit the congressional agenda, there is often a vast mobilization of dozens of congressional bodies, each claiming some small piece of the action.

The analyses already presented provide ample ev-

TABLE 7

The Relationships between the Percentage of Hearings Held in the Dominant Venue and the Number of Bodies Conducting Hearings in a Given Year, 1945–86

	EQUATION 1 (N = 42)				EQUATION 2 (N = 41)				
POLICY AREA	R^2	INTERCEPT	NUMBER OF BODIES	D-W	R^2	INTERCEPT	NUMBER OF BODIES	LAGGED % OF HEARINGS IN DOMINANT VENUE	BREUSCH-GODFREY TEST
Nuclear power									
Linear	.68	51.72	−1.41*	1.89	.67	48.92	−1.33*	.05	−.05
		(2.83)	(.15)			(7.72)	(.24)	(.14)	
Log	.57	68.01	−15.80*	1.25[a]	.61	51.16	−12.13*	.26	.59
		(5.39)	(2.15)			(10.04)	(2.80)	(.13)	
Drug abuse									
Linear	.49	45.60	−.93*	1.27[a]	.63	33.16	−.68*	.30*	−.77
		(2.99)	(.15)			(7.43)	(.20)	(.15)	
Log	.57	53.93	−11.01*	1.29[a]	.70	42.84	−8.93*	.25	−.30
		(3.52)	(1.50)			(7.36)	(1.83)	(.13)	
Smoking									
Linear	.27	42.47	−2.21*	1.67	—	—	—	—	—
		(4.61)	(.57)						
Log	.30	55.75	−15.95*	1.63	—	—	—	—	—
		(7.32)	(3.84)						
Pesticides									
Linear	.47	46.70	−2.11*	1.63	—	—	—	—	—
		(3.14)	(.36)						
Log	.55	51.64	−13.00*	1.87	—	—	—	—	—
		(3.34)	(1.88)						

Note: The dependent variable in all equations is the yearly index of hearings on a given topic held in the dominant venue. Entries are unstandardized regression coefficients (standard errors in parentheses). Equation 1 estimates a model without lags: $(Y_t = a + b_1x_t + e_t)$. Equation 2 estimates a model with a lagged endogenous variable: $(Y_t = a + b_1x_t + b_2y_{t-1} + e_t)$.

[a] Test indicates potential autocorrelation. Equation 2 model shows no autocorrelation problem.

*$p < .05$, two-tailed test.

idence of the policy importance of the changes in jurisdictional control that follow from the expansion of conflict shown in Table 7. Tables 1–5 showed how competing groups of congressional committees (what we have termed competing venues for policy action) harbor distinctly different biases. Now we can see the importance of agenda setting and of issue definition in Congress. Where issues are clearly within the agenda of only a single group of like-minded committees, policy is likely to follow the wishes of its members. Where the issue rises on the agendas of other congressional bodies, however, this is likely to bring an end to the dominant policy bias. New committees will consider different—and often less flattering—aspects of the issue, and the policy bias of legislation may change dramatically.

In summary, the conflict expansion process within Congress appears to follow a clear pattern. Increases in attention to an issue lead to great demand to be heard in Congress and to increases in the number of distinct congressional bodies active in the area. Since the rise in congressional attention is usually associated with some changes in how the issue is defined, there are ample grounds for jurisdictional aggrandizers within Congress to claim some part of the action. The increase in opportunities to be heard rarely comes only from those committees with established jurisdiction; rather, many congressional bodies argue that they have say over some part of the issue. Table 6 showed that congressional committees are often successful in claiming jurisdiction over at least some part of a newly important issue. This process of jockeying for control inevitably challenges the prevailing equilibrium of institutions and interests, and we see clear evidence in Table 7 that the expansion of bodies holding hearings is strongly related to a breakup in jurisdictional control. Evidence presented in Tables 1–5 showed the likely policy consequences of these changes in control. The dynamics of changing committee jurisdictions make Congress not only the home of cozy arrangements between members of policy subsystems but also the institution of government through which policy monopolies can be destroyed.

CONCLUSION

We have reconfirmed important aspects of what we have termed the theory of committee statics, using a novel methodological approach. Specifically, we have presented new evidence that supports the contention of policy bias in congressional committees. We have seen that committees that are favorable to an economic interest tend to hold hearings that are positively skewed toward that interest. In the two cases where we coded thousands of witnesses appearing in hundreds of congressional committees, we observed a marked tendency for favorable venues to specialize in favorable witnesses.

This information alone is not enough to impute bias to the legislative process. Committee bias is important for controlling legislative agendas only when committees can establish a jurisdictional monopoly over an issue. Where several committees consider different aspects of an issue, the jurisdictional monopoly has broken down, and bias is far less likely. There is a dynamic associated with the creation and dissolution of jurisdictional monopolies. In particular, when issues become more controversial, jurisdictional monopolies are likely to come under fire. For the issues we studied, as attention (the number of hearings held on an issue) increased, the number of committees involved in scheduling hearings increased. And as interest in the issue expands, the percentage of hearings favorable to the existing policy subsystem declines, and the number of hearings conducted in unfavorable committee venues increases. Our dynamic regression analyses demonstrate the fragility of monopolistic control over issues in Congress.

The existence of jurisdictional dynamics would seem to pose serious problems to the existing equilibrium-based analyses of committee structure. At the minimum, rules must be flexible enough to allow for new understandings of issues; otherwise, we would not have observed the process of disruption of jurisdictional monopolies. Our analysis also poses problems for Krehbiel's informational model, in which committees collect and package information for the chamber. Committees do not just collect information and package it for the chamber; they collect biased information and themselves demonstrate marked tendencies to favor issue definitions that are consistent with the imputed bias of the committee venue.

We have elsewhere proposed a punctuated equilibrium model of the agenda-setting process in the United States (Baumgartner and Jones 1993). That model stresses the existence of periods of stability characterized by jurisdictional monopolies and consensual understandings of issue dimensions. These periods of stability are punctuated by periods of rapid change characterized by the intrusion of new issue understandings and new participants in the policy process and by the breakup of previously existing policy monopolies. During periods of stability, issue understandings are reinforced through the congressional hearing process. In a recent article discussing the structure-induced equilibrium literature in Congress, Dion (1992) has formally analyzed those factors that can help to create or to destroy equilibria (e.g., the indifference of members). We conceptualize a system in which new institutional structures are created from time to time, each harboring a new set of biases. Cross-sectional views of these biases are of course interesting, but they cannot tell us whether the biases and the structures that create them might change over time. We believe that they change in dramatic ways over time and that this can best be observed by examining the hearings process. Hearings provide multiple functions in this system, including the provision of symbolic outputs for the policy community, the raising of attention on the issue, and the depicting of acceptable solutions that committees hope will influence the bill-writing process. That is, much occurs in hearings that has little to do with the drafting of legislation but that is critical to the process of issue definition, a point quite unappreciated in the recent literature on committees and on Congress more generally.

These issue definitions are occasionally subject to challenge, and these challenges punctuate the more normal periods of policy subsystem equilibrium. In the issues we studied, the environmentalists challenged the "atoms for progress" image of nuclear power (Baumgartner and Jones 1991; Weart 1988) and the "agricultural bounty" image of pesticides (Bosso 1987). Government health agencies attacked the tobacco industry, with many successes but without victory (Fritshler 1989). And the dominant enforcement-based drug policy subsystem was challenged by social service professionals during the Nixon administration (Baumgartner and Jones 1993; Falco 1989; Musto 1987; Sharp 1991, n.d.). In each case, as new participants brought fresh issue understandings to the policymaking process, more and different committees held hearings on the topics. The content of the hearings changed, becoming more hostile as attention increased and shifted from one dominant group to another. While the approaches of committee statics are appropriate for the periods of stability, they are not so useful for understanding the periods of struggle over issue definition and jurisdictional monopolies. This struggle is fundamental to understanding the policy process.

Notes

This is a revised version of a paper originally presented at the 1992 meetings of the Midwest Political Science Association. We appreciate the helpful comments we received from Charles Franklin, David King, and Dan Wood.

1. In the four cases we discuss, only drug abuse requires a slight reconceptualization, since none of the hearings are favorable; in that case, we differentiate between enforcement (including interdiction and border controls) and treatment (including education, rehabilitation, and health topics). For other topics (e.g., urban affairs), the simple dichotomy cannot be used. For more details on coding, see Baumgartner and Jones 1993, esp. app. A.

2. In these regressions, the unit of analysis is the year, not the hearing. The total number of hearings on which these data are based are reported in Tables 3–5. When we run the identical regression based not on the number of committees but on the number of distinct *subcommittees*, the results are

even slightly stronger in the proportion of variance explained. For simplicity, we present only one model here.

3. The standard test for autocorrelation, the Durbin–Watson (d) statistic, is not appropriate when lagged endogenous variables are used; we present Durbin's h in equation 2 of Table 6 (see Gujarati 1988, 525–27; Ostrom 1990, 66.).

4. In a few cases, as indicated by missing data points in Figures 1 and 2, the percentage of hearings conducted in the dominant venue could not be calculated because there were very few or no hearings in a given year. In those cases, we have coded the maximum score of 50 in our monopoly control variable, since *no hearings* is a sign of strong control. In any case, recalculating the figures in Table 7 with these data missing shows only minor fluctuations from the results reported.

5. Durbin's h-test for autocorrelation is indeterminate in certain cases, in which cases the Breusch–Godfrey test is appropriate (Johnston 1984, 319–21; Ostrom 1990, 66). We report this figure in Table 7.

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ANXIETY, ENTHUSIASM, AND THE VOTE: THE EMOTIONAL UNDERPINNINGS OF LEARNING AND INVOLVEMENT DURING PRESIDENTIAL CAMPAIGNS

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By incorporating emotionality, we propose to enrich information-processing models of citizens' behavior during election campaigns. We demonstrate that two distinct dynamic emotional responses play influential roles during election campaigns: anxiety and enthusiasm. Anxiety, responding to threat and novelty, stimulates attention toward the campaign and political learning and discourages reliance on habitual cues for voting. Enthusiasm powerfully influences candidate preferences and stimulates interest and involvement in the campaign. The findings support a theoretical perspective that regards cognitive and emotional processes as mutually engaged and mutually supportive rather than as antagonistic. We suggest that the democratic process may not be undermined by emotionality as is generally presupposed. Instead, we believe that people use emotions as tools for efficient information processing and thus enhance their abilities to engage in meaningful political deliberation.

> Fear is associated with the expectation that something destructive will happen to us. . . . People do not believe this when they are, or think they are, in the midst of great prosperity, and are in consequence insolent, contemptuous, and reckless . . . nor yet when they have experienced every kind of horror already and have grown callous about the future [for] there must be some faint expectation of escape.
>
> . . . Fear sets us thinking what can be done, which of course nobody does when things are hopeless. Consequently, when it is advisable that the audience should be frightened, the orator must make them feel that they are really in danger. —Aristotle, *Rhetoric* 2.5.1383

We would like to suggest that emotion is a catalyst for political learning. In particular, the analyses we shall present argue that threat powerfully motivates citizens to learn about politics. On the face of it, our proposition makes too much sense to ignore. Generally inattentive to political matters, citizens may require sharp notice before they become motivated to learn anything new. And at least at the intuitive level, threat seems as good a spur to action as any. In addition, we suggest that the ability of political leaders to generate enthusiasm stimulates political involvement. This second claim has a long-standing and long recognized status (e.g., Schattschneider 1960). More deeply, we believe that a mounting body of evidence in neurophysiology, psychology, and political science points toward the distinctive roles that different emotions play in stimulating political attentiveness.[1] We offer a view that shows how emotionality aids, rather than disrupts, political reasoning and enhances, rather than diminishes, the quality of democratic life.

We shall report a series of empirical tests that establish the importance of anxiety and enthusiasm for political learning and involvement, respectively. First, we demonstrate that fear (anxiety) and enthusiasm are distinctive emotional responses to political candidates and thereby eliminate a simple "valence" view of emotions. Second, we observe that people's anxiety and enthusiasm varies with political events and is not a permanent feature of individual personalities. Third, we consider evidence that anxiety and enthusiasm play distinctive parts in the voting decision. Fourth, we show explicitly that anxiety, rather than enthusiasm, moves people to learn policy-related information about candidates. More generally, we argue that anxiety works cooperatively with learning to shift attention to political matters and to diminish reliance on habit in voting decisions. Finally, in a parallel analysis, we show that enthusiasm, rather than anxiety, has a distinct effect on political involvement.

THEORETICAL BACKGROUND

The idea of threat as an attention-getting device makes common sense. Hit it over the head with a two-by-four and you can get the attention of even a mule. Nothing focuses the mind so well as the prospect of one's own hanging. And so on.

Anxiety also occupies a prominent place in the contemporary psychology of emotions. Over the past decade, psychologists have developed a two-dimensional typology of emotional response that clearly distinguishes anxiety from such emotions as depression (e.g., Ax 1953; Diener and Emmons 1985; Plutchik 1980; Russell 1980; Tellegen 1985; Watson and Tellegen 1985; Zevon and Tellegen 1982). At the same time, the two-dimensional character of emotional response has proven a powerful schema for the analysis of citizen response to political candidates

(Abelson et al. 1982; Marcus 1988b; Masters and Sullivan 1989; Sullivan and Masters 1988).

Parallel evidence lies in current neurophysiology. It is now widely understood that the human brain's limbic system has two subsystems, each of which generates distinctive emotional responses (Eccles 1989; Fonberg 1986; Gray 1981, 1987a, 1987b). One subsystem generates emotions that fall in the class of excitement, elation, and enthusiasm; the other subsystem generates emotions that fall in the class of anxiety, stress, and fear. The combined outputs of these systems generate the mood state (forming what is most often described as a *circumplex*). We have elsewhere described more fully the circumplex model and the supporting literature in psychology (Marcus 1991; Marcus and Rahn 1990). It is important to emphasize that this model describes mood—and changes in mood—as two-dimensional. Mood states are an *amalgam* formed by two distinct physiologically based systems of arousal, each of which influences specific gradations of mood that we readily recognize and to which we assign everyday labels (Storm and Storm 1987).[2]

Especially intriguing is the neurophysiological work on the strategic functions played by distinct emotional responses. Each of the two systems—that of anxiety and that of enthusiasm—appears linked to behaviorally different sorts of psychic orientation.

Consider first the threat-attendant system that generates moods ranging from safety to anxiety. Feeling calm, placid, and secure indicates the absence of threat; feeling apprehensive, fearful, or in dread indicates the presence of threat. According to Gray's (1987b) model of anxiety, this system operates to interrupt ongoing activity. It does not control subsequent behavior; rather, it arrests ongoing activity and enables other control systems—cognitive and emotional—to respond (cf. Simon 1967). More specifically, the *behavioral inhibition system* continually matches incoming sensory stimuli against contemporary plans and expectations. As long as the comparisons continue to confirm the safety of the environment, moods of calmness and safety prevail and ongoing actions are left undisturbed. However, if a "mismatch" occurs, then ongoing activity is inhibited, attention is shifted toward the intrusive source, and increased arousal occurs. Put more plainly, the appearance of a novel or threatening intrusion causes us to stop, look, listen, and get ready for action.

Anxiety, as we use the term, is not the sort of primitive emotion that underlies the fight/flight system (Gray 1987b).[3] In the realm of electoral politics, candidates and parties may anger, disgust, and threaten fundamental values and beliefs of voters. Yet they do not present physical dangers that engage the instinctive, reptile-brain-centered responses that operate independent of cognition. Instead, these threats endanger symbolic worlds, environs of values and beliefs, the stuff of contemporary mass politics (Edelman 1964). Thus, the emotional responses that we label "anxiety" reflect mechanisms that already join cognitions with emotions. Indeed, the two emotionality systems are cognitive systems that make apprehensions manifest as feelings.[4]

Experiments in cognitive psychology demonstrate that negative events increase attention and that emotional reactions are crucial to the stimulation of attention (Derryberry 1991; Pratto and John 1991). Thus, current work in psychology and in neurophysiology supports a theoretical view about how people come to learn about politics: they abandon complacency and start to pay attention when the world signals that something is not right.

The second class of emotional arousal monitors current behavior. This system generates moods of enthusiasm or elation as our personal tasks and social activity succeed and generates moods of melancholy or depression as we experience failure. The *behavioral approach system* provides active feedback of our ongoing behavior and marshals the physical and mental resources necessary for success. These moods are essential for the proper performance of learned behavior. The variance in moods generated by the behavioral approach system provides an important marker for the strengthening or wavering of motivation. Thus, for politics, we ought to find that variations in enthusiasm ought to predict variations in political involvement. More precisely, during political campaigns, candidates must generate enthusiasm for themselves among voters in order to gain their support and to create active interest in the election. When voters respond to a candidate with enthusiasm, they are not merely evincing passive sympathetic reactions but sharing convictions and commitment to common endeavors. Rather than stopping, looking, and listening, enthusiasts throw themselves into the cause.

During the past two decades, political science, as well as psychology, has concentrated on matters of cognition. We have studied opinions, beliefs, and values and refined concepts such as ideology, belief system, and schema. The basic presumption has been that understanding what people know and how they come to know it will explain what people do. To be fair, this sustained effort has produced a substantial increase in our fundamental understanding about political cognition. Yet the returns from further elaboration of the information-processing model appear to be diminishing (Kuklinski, Luskin, and Bolland 1991; Markus 1986). To move forward, we need to extend the information-processing model beyond the contemporary restriction to cognition. Thus, we turn to matters of emotion—in particular, the roles of anxiety and enthusiasm.

We suggest that people rely on their feelings to provide them with important strategic information. More than coloring cognitions with values, changes in mood constitute a critical part of information-processing mechanisms (Cacioppo et al. 1986). We aim to demonstrate that this particular view (which emphasizes the role of *anxiety* in information processing) uniquely contributes to our understanding of political matters. We shall develop and test hypotheses about political information processing that depend crucially on emotional response.

TWO TYPES OF EMOTIONAL RESPONSE

The empirical work in both mood psychology and neurophysiology indicates that we should expect two types of emotional response, which we call "anxiety" and "enthusiasm." Our first empirical steps show that political candidates elicit these two sorts of responses in the mass public. Here we extend work already done (Abelson et al. 1982; Marcus 1988b) by adding two new twists. First, we observe that the "dual-system" view of emotional response stands up under different measurement techniques. In so doing, we eliminate the alternative "valence" hypothesis about the structure of emotional response. Second, we observe that this dual system is not stable but instead reacts to the psychic pressure of the campaign. Thus we support an understanding that emotional responses are functionally focused, with one system alert to intrusive signals of novelty and threat and the other system monitoring the success of current behavior.

In order to test the dimensionality and the dynamics of emotional response, we examine two data sets. The first is the familiar American National Election Studies (ANES) panel of 1980, with interviews taken in January, June, and October. The second, a commercial survey, represents the views of Missourians during the 1988 presidential campaign in a series of three cross sections taken during June, July, and October. We are fortunate in that the ANES staff included seven emotional response items in the 1980 panel, eliciting a variety of emotional responses. In each wave, respondents were asked: "I am going to name a political figure, and I want you to tell me whether that person, or something he has done has made you have certain feelings like "anger" or "pride," or others I will mention. Think about Jimmy Carter. Now, has Carter—because of the kind of person he is, or because of something he has done—ever made you feel: angry?" The respondent was then asked whether Carter had made him or her feel "hopeful," "afraid of him," "proud," "disgusted," "sympathetic toward him," and "uneasy." The same sequence was repeated for Reagan (and then other candidates). While "anxiety" is not included as one of the response items, we expect that the terms *afraid, uneasy, anger* and *disgust* will serve as appropriate markers. Similarly, though "enthusiasm" is not included, we expect that the terms *proud, hope,* and *sympathy* will be appropriate markers for this dimension.

While the seven items were not explicitly designed to distinguish anxiety from enthusiasm, they represent the data base for previous work on dimensionality (see Abelson et al. 1982; Kinder et al. 1980; Marcus, MacKuen, and Glassberg 1989), and they surely approximate our intent.

We begin our work by examining how people reacted to the candidates. If our dual-system understanding of emotional response is correct, then we should see some evidence of the candidates' stimulating a *combination* of emotional responses. On the other hand, if the conventional "valence" understanding is correct, then the candidates should produce a single emotional response (like vs. dislike). Thus, we want to see if Carter and Reagan got people to experience (1) a *combination* of enthusiasm and anxiety or (2) a sense of enthusiasm as *opposed* to anxiety.

A factor analysis of the seven items moves us forward in two ways. First, it permits us to see whether the enthusiasm items hang together and the anxiety items hang together. This is a measurement issue, a matter of fundamental importance. Second, the factor analysis provides a weak test of the valence versus dual-system view of emotional response. The valence model predicts that the enthusiasm and anxiety items will line up in polar opposites; the dual-system model predicts that enthusiasm and anxiety need not be—and are unlikely to be—polar opposites.

A straightforward factor analysis of the seven items (here from the January reading, i.e., before the campaign began) suggests the plausibility of a two-dimensional view. Figure 1 presents the factor space. The data clearly sustain our measurement requirement that the enthusiasm items and the anxiety items separate into distinctive clusters. Further, the pattern clearly defeats the valence theoretical view: the enthusiasm and anxiety clusters do not line up as polar opposites.

Yet the nature of the 1980 ANES survey questions does not allow us to eliminate an alternative understanding. The data could still prove consistent with the valence model under a subtle but plausible interpretation. It is possible that individuals respond in conventional valence terms (positive opposed to negative) while the second dimension represents the intensity of the emotional response (see Larsen, Diener, and Cropanzano 1987; MacKuen 1987; Russell 1980).

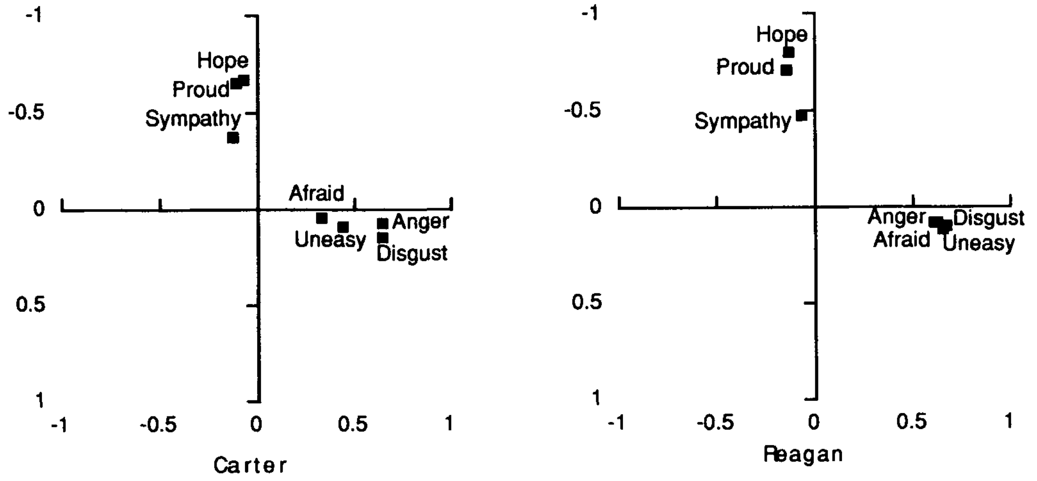
In order to bring evidence to bear on this matter, we designed a special-purpose question wording to tap each of the two dimensions. We chose appropriate word markers to elicit responses most closely associated with each of the two dimensions (Watson, Clark, and Tellegen 1988). Importantly, we ensure that a respondent can report (1) an absence of emotional response toward a candidate on one or both dimensions of emotionality and (2) intermediate degrees of response, for example, a sense that the candidate was somewhat (or very) calming or boring.[5]

We presented the respondent with a modified feeling thermometer anchored by pairs of words connoting anxiety versus safety or, alternatively, enthusiasm versus depression. In order to obtain a minimal validation test, we chose two pairs for each dimension. The enthusiasm pairs were (*enthusiastic* vs. *unenthusiastic*) and (*interested* vs. *indifferent*) and the anxiety pairs were (*upset* vs. *comfortable*) and (*anxious* vs. *safe*). For an enthusiasm example, consider the following:

> When we talk to people about the major Presidential candidates, they use different words to describe how they feel about them. For both Vice President Bush and Governor Dukakis, I'd like to read you some pairs of

FIGURE 1

Factor Space of Seven Affect Terms Used To Map Emotional Responses to the 1980 Presidential Candidates



Source: 1980 ANES data.

Note: The figures represent a varimax rotation of a principal factor solution for the correlation matrix among the seven items for each candidate. The dimensionality of a factor space is not, of course, a simple statistical inference. Here, it appears that two dimensions capture the bulk of the common variance. The eigenvalues for Carter are 1.48, .74, and .14. For Reagan, the eigenvalues are 2.07, 1.03, and .35.

> words. For each pair, let's use one [1] for the lowest possible rating and 100 as the highest possible rating.
>
> Let's start with Vice President Bush. Would you say you feel "unenthusiastic" or "enthusiastic" about him? One [1] would be the most unenthusiastic rating and 100 would be the most enthusiastic rating.

We then piggybacked our emotion-thermometer items onto a commercial poll in the state of Missouri during three periods of the 1988 presidential campaign. The first wave, in June, followed the Missouri primary by three months and represents a period of relative calm in the local environment. The second wave, in July, immediately followed the Democratic National Convention and represents the high point for the Dukakis campaign. Finally, the third wave, in late October, measures emotional response at the end of the national campaign.[6]

A similar factor analysis (here for the relatively quiet June period) of the four new items produces Figure 2. Note that the two item pairs fall neatly into two distinct clusters: anxiety and enthusiasm.[7] Further, the distinctiveness of the emotions is apparent. Were anxiety and enthusiasm antipodes, the four items would line up along one dimension, with *enthusiasm* and *interest* at one end and *anxious* and *upset* at the other end. This is obviously not so since the enthusiastic–unenthusiastic and interested–indifferent ratings are nearly orthogonal to the upset–comfortable and anxious–safe ratings. Thus in these new measures, the enthusiasm and anxiety measures are not mere opposites, as the valence view of emotional response would predict; instead, they appear to be separate entities, as the dual-system view expects.

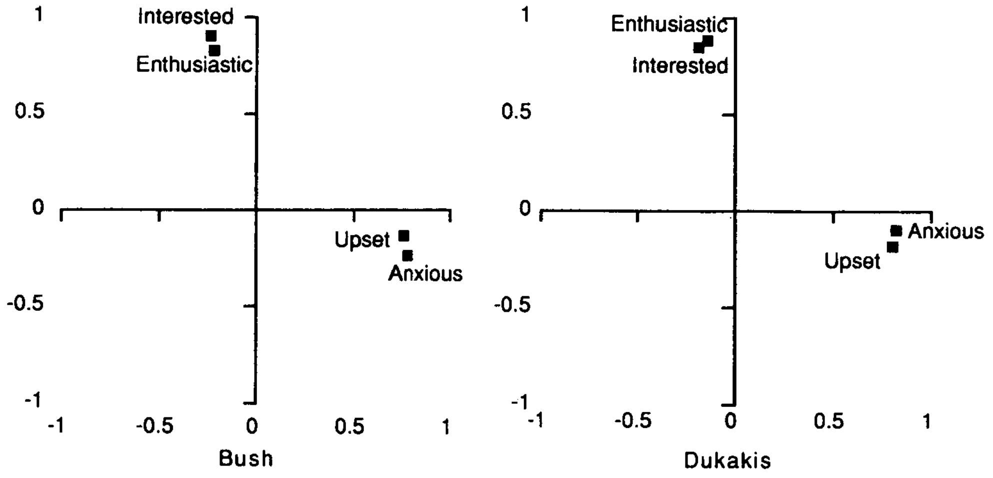
In the end, the factor-structural evidence rejects the hypothesis of a single valence dimension and instead supports the current view that anxiety and enthusiasm are distinctive emotional responses. Yet this evidence should not persuade. So far, we observe only static correlation, a matter of *which* emotional responses go together, rather than evidence of theoretical function. More persuasive evidence would require that we demonstrate that each dimension of emotional arousal has systematic and distinct behavioral consequences congruent with the dual-system theory. That requires that we show that one distinct behavior, learning, is influenced by changes over time in moods of anxiety and that another distinct behavior, political involvement in the campaign, is influenced by changes over time in moods of enthusiasm. We turn to the dynamic relationships between political events, mood responses, political learning, and political involvement.

THE DYNAMICS OF EMOTIONAL RESPONSE

People's emotional responses react to the ongoing campaign. As the winds of the campaign shift one way and the next, so do emotional responses. The evidence on dynamics is crucial for testing the validity of our theoretical view. We posit that emotions enhance people's ability to interact with the environment. To be effective, these emotions cannot be permanent features of an individual's personality or of a candidate's image. Only when emotions reliably react to changes in the informational environment (i.e., to campaign news) can they encourage citizens to become engaged with their favorite candidate's prospects or, more interestingly, interrupt citizens'

FIGURE 2

Factor Space of Four Affect Terms Used To Map Emotional Response to the 1988 Presidential Candidates



Source: 1988 Missouri Data.
Note: The figures represent a varimax rotation of a principal factor solution for the correlation matrix among the four items for each candidate. Again, it appears that two dimensions capture the bulk of the common variance. The eigenvalues for Bush are 2.09, .74, and .04. For Dukakis, the eigenvalues are 1.96, .94, and .02.

ordinary political activity and spur information processing.

That people react emotionally to the campaign is easily shown. Here we create simple scales of anxiety and enthusiasm by taking each individual's responses to the relevant items and norming to a zero–one interval.[8]

Table 1 shows how the public's emotional reactions reflected the events of the 1980 and 1988 campaigns. Each entry is the amount of anxiety or enthusiasm that each candidate (the column heads) elicited from the public. For example, in January 1980, about 40% of the public volunteered terms such as *uneasy* or *disgusted* to describe their reactions to Carter. Following severe failures in both economic and foreign policy (a spectacular inflation scare, rising unemployment, the enduring hostage crisis), this portion climbed to 53% by June and maintained that level for October. Reagan avoided such reactions through June and only began to generate uneasiness when brought under attack during the fall campaign. We observe a similarly transparent pattern in 1988: the July survey, taken directly after the Democratic National Convention, shows a high level of anxiety about Bush. By October the anxiety about Bush has receded, while the survey reveals the public's disquietude about Dukakis after that fall's pointedly "negative" campaign.[9]

None of this is entirely remarkable by itself. Instead, it demonstrates that a sense of anxiety is not a permanent feature of the political landscape but a dynamic one, closely linked to prominent external events.[10] It is, however, weak evidence at best; it merely indicates that the two emotional systems operate independently. We next turn to more crucial and demanding tests.

TABLE 1

Aggregated Means of Emotional Response over the 1980 Presidential Campaigns

TIME OF SURVEY	ENTHUSIASM	ANXIETY	ENTHUSIASM	ANXIETY
1980 Presidential Campaign[a]	Carter		Reagan	
January	.65	.40	.29	.20
June	.58	.53	.38	.25
October	.52	.50	.39	.39
1988 Presidential Campaign[b]	Bush		Dukakis	
June	.40	.52	.49	.48
July	.27	.55	.53	.45
October	.50	.43	.42	.56

[a] *Source*: 1980 ANES.
[b] *Source*: 1988 Missouri data.

TABLE 2

Estimating Presidential Preference 1988 during Three Waves: Multivariate Model

INDEPENDENT VARIABLES	REGRESSION COEFFICIENTS AND STANDARD ERRORS JUNE	JULY	OCTOBER
Comparative enthusiasm	1.16*	1.04*	1.07*
	(.10)	(.09)	(.10)
Comparative anxiety	−.00	−.10	−.05
	(.10)	(.09)	(.09)
Partisanship	.35*	.31*	.35*
	(.07)	(.06)	(.08)
Constant	−.29*	−.15	−.26*
	(.09)	(.08)	(.09)
Number of cases	253	247	246
Adjusted R^2	.59	.68	.64
Root Mean Square Error	.29	.25	.28

Source: 1988 Missouri Data.
Note: Voting preference indicates Dukakis or Bush supporters (scored 1 and 0), "leaners" (.75 or .25), and undecided (.50). All variables are scored to a common range of 0–1. (See Appendix.) The entries are unstandardized regression coefficients with standard errors in parentheses.
$^*p \leq .05$, two-tailed test.

EMOTIONAL RESPONSE AND THE VOTING DECISION

Understanding that anxiety and enthusiasm represent structurally and dynamically distinctive emotional responses carries us only part way. We shall show that anxiety and enthusiasm play importantly different roles in the voting decision. In particular, the data indicate that enthusiasm directly affects voting preference (reflecting something very close to the voting decision itself), while anxiety has practically no direct impact on choice. Equally important for our point of view, anxiety appears to give voters pause—to get voters to base their decision on candidate characteristics or campaign information rather than merely stick with their "standing choice."

Consider first the relative power of enthusiasm and anxiety on voting preferences. The standard "valence" view of emotion would predict that emotions will affect voting preference directly. More to the point, this view expects enthusiasm and anxiety to affect those preferences equally. Our theoretical position, that anxiety focuses attention while enthusiasm moves psychic involvement, suggests that enthusiasm will directly affect the voting decision while anxiety's role will be muted. Thus, an evaluation of voting preference as a function of the two distinctive emotions will tell the tale. If both emotions play about equal parts, then the standard view prevails. If enthusiasm is more important than anxiety, then the dual-system view stands stronger.

Table 2 presents simple voting equations, one for each of the three waves in 1988.[11] In each case, presidential preference is regressed on partisanship (in Key's terms, the standing voting decision) and the crucial measures of comparative enthusiasm and comparative anxiety.[12] A quick look tells the story. Enthusiasm matters enormously, anxiety not at all. For all three waves, the parameter for enthusiasm is both substantial and statistically significant. For all three waves, the parameter for anxiety is invisible. Clearly, enthusiasm leads the way in guiding vote choice. Importantly, the data substantiate the pattern of results in a similar (though more elaborate) analysis of voting in the 1984 election (Marcus 1988b).[13] This, of course, does not by itself indicate that the dual-system view prevails. We have merely shown that anxiety plays a decisively different role than does enthusiasm. If our view is correct, then we should expect that the voting calculus will differ for those who perceive threat in the environment than for those who remain calm.

The behavioral inhibition system is rarely intrusive, because we are infrequently confronted by threat or sudden surprise. The effect of the anxiety system will be manifest only when a threatening stimulus is apprehended. This suggests that the influence of negative affect is sporadic, not constant. When threat is low, the behavioral approach system governs action: we go forward when our enthusiasm increases and withdraw when we sense frustration and exhaustion. However, when we feel threatened, we set aside habits and focus attention on the problematic.

Because a political campaign is a struggle between competing partisans, some citizens, though not all, experience the cut-and-thrust of politics as threatening. People unaroused will safely vote their standing choice while those pricked by anxiety will perk up, gather new information, and perhaps abandon their old habits.

For evidence, look at Table 3. Here we model vote preference as a function of comparative enthusiasm and partisanship (as in Table 2) as well as anxiety's effect on the role of comparative enthusiasm and partisanship. In this equation, we introduce the respondent's total anxiety meaned over both candidates (as opposed to the comparative anxiety measure in Table 2) to measure the amount of environmental threat. (Note that someone greatly, but equally, uneasy about both candidates will produce a comparative anxiety score of zero but, properly, a high total anxiety score.) Because the behavioral inhibition system responds to threat, our dual-system theory predicts that the presence of anxiety will cause people to drop partisanship as a sure guide to candidate choice and to turn to candidate-specific information for judgment.

We estimate the direct effects and the crucial conditional effects when we write explicit interaction terms (in rows 2 and 4). We see that the presence of anxiety increases the importance of comparative enthusiasm (.62) and diminishes the role of partisanship (−.60). In fact, high anxiety almost eliminates partisanship as a consideration.[14] As the dual-system

TABLE 3

Estimating Presidential Preference 1988: Anxiety's Effect on the Role of Enthusiasm and Partisanship

INDEPENDENT VARIABLES	COEFFICIENTS	STANDARD ERRORS
Comparative enthusiasm	.79*	(.12)
Anxiety * comparative enthusiasm	.62*	(.22)
Partisanship	.64*	(.11)
Anxiety * partisanship	−.60*	(.21)
Constant	−.25*	(.02)
Sample size	746	
Adjusted R^2	.65	
Root Mean Square Error	.27	

Source: 1988 Missouri Data.

Note: For comparability, all variables are scored to a common range of 0 to 1. See Appendix. Voting preference indicates Dukakis or Bush supporters (scored 1 and 0), "leaners" (.75 or .25) and undecided (.50). The anxiety interactions (in rows two and four) represent multiplicative interactions. Anxiety is the voter's mean anxiety (over both candidates). The values are unstandardized regression coefficients with standard errors in parentheses.

$^*p \le .05$, two-tailed test.

theory predicts, a rise in anxiety *weakens* the reliance on partisanship and *strengthens* the reliance on contemporary emotional reactions to the candidates. A drop in anxiety (i.e., an increase in complaisance) strengthens the impact of partisan identification and weakens reliance on concurrent feelings of enthusiasm toward the candidates.

Thus, the two emotions matter for voting but matter in different ways. Comparative enthusiasm affects how closely people are willing to embrace either candidate. Anxiety plays a very different role: it stimulates peoples' attention and releases them from their standing decisions.

DIRECT EVIDENCE ON LEARNING

The evidence suggests that threat stimulates learning.[15] Yet, it is circumstantial evidence. All we have established to this point is that anxious voters are less reliant on habit. For a more direct test, we need to observe how people's political knowledge changes over time. We turn to the 1980 ANES panel.[16]

Over the course of any campaign, citizens acquire and develop views about candidates. From January to October in 1980, the public developed an increasingly rich portrait of the challenger, Reagan. The portion claiming to know something about him rose from 86% to 95%, the portion willing to evaluate his personal characteristics rose from about 60% to 90%, and the portion identifying his position on policy questions rose more modestly from about 50% to 70%. All these gains made Reagan almost, but not quite, as familiar as the incumbent Carter (see also Markus 1982; Miller and Shanks 1982).

Yet cognitive elaboration is not the same thing as learning.[17] Hence, we need a measure of what people know about politics and, more decisively, a measure of what they know that is relevant for their vote choice. Here, we use a device, used elsewhere, that concentrates on what is deemed to be objectively true. To be brief, we measure knowledge by the respondent's ability to say that Ronald Reagan is more conservative than Jimmy Carter. Each individual obtains a "knowledge" score that counts the number of times, on a set of seven-point issue scales, that the individual placed Reagan to the right of Carter.[18]

As measures of political learning, these policy-related cognitions have several useful features. First, they are relatively unambiguous. Compared with prompts about candidate traits or open-ended responses about political objects, respondents who manufacture cognitions can be found out. Second, they represent important and easily available political facts. Information that Reagan was more conservative than Carter could be easily obtained from either the mass media or from conversations with political knowledgeables. The public, when aggregated, had little trouble seeing that Reagan was well to the right of Carter on every one of these issues. Finally, such elementary policy-related knowledge is crucial in the link between voting and public policy. In fact, it is hard to imagine that anyone who paid attention to the 1980 campaign could have escaped this information.

Of course, many did. Table 4 displays the proportion, corrected for guessing, of the public who positioned Reagan to the right of Carter on three central policy questions as well as on the liberal–conservative continuum. The proportions are given for samples taken in January, June, and October of 1980. First, observe the overall levels; substantial numbers of the electorate, even in the end, remained unaware of the candidates' policy differences.

In learning terms, however, note that the public began to see the policy distinctions more and more clearly as the campaign progressed. Most striking, when the campaign began only 13% saw Reagan as more committed to defense spending, but when the season turned to fall, fully 51% realized what was going on. The public similarly gained understanding about the candidates' stances on the spending-and-social-welfare and détente issues as well on the ideological spectrum. The row of numbers across the bottom shows a composite measure, the means for proper placements on the three issues and for ideology. Overall, it looks as though the campaigners' efforts to "inform" the electorate had a salutary, though modest, effect.

Our question is whether this learning was motivated by emotions. After all, other plausible learning mechanisms abound. To proceed, we shall control for powerful alternative hypotheses when we estimate the amount of learning that might be attributed to anxiety.

Start with a cognitive model. As ever, education matters. Surely college-educated, rather than grade-school-educated, people can better extract issue-oriented information from the hurly-burly of campaign rhetoric. To education, add interest. We now under-

TABLE 4

Knowledge about Candidate Policies over Time

POLICY AREA	CORRECTED PROPORTION SAYING REAGAN MORE CONSERVATIVE THAN CARTER[a] JANUARY	JUNE	OCTOBER
Defense spending	.13	.34	.51
Detente with Soviets	.21	.28	.37
Cut spending/social programs	.29	.29	.34
Liberal–conservative continuum	.19	.35	.34
Summary measure[b]	.21	.32	.39

Source: ANES 1980 Data.
Note: The standard errors of the means are about .02.
[a]Proportion placing Reagan to the right of Carter minus the proportion placing Carter to the right of Reagan.
[b]The mean score for all items.

stand that the already well informed and motivated will be most likely to learn (Neuman 1986; Tichenor, Donohue, and Olien 1970). Having a knowledge base both marks a more permanent interest in, and capacity for, politics as well as provides the framework in which new information can be integrated to produce increments in knowledge. After all, information about presidential candidates fills the air: learning requires not a search for information but instead an inclination to pay attention to, and make sense of, what is readily available.

Next, add in partisanship. Strong partisan attachments should enable individuals to make correct inferences about the political world that might otherwise be impossible. Brady and Sniderman (1985) show that individuals use an affect-heuristic that assumes that friends (liked social and political groups) have compatible political views while opponents (disliked others) have different political views. Understanding candidate stances is, for the most part, a matter of inference rather than knowledge. The Brady–Sniderman hypothesis, in a way familiar to "new look" psychology of the 1940s and 1950s (e.g., Heider 1958; Rosenberg and Abelson 1960), suggests that citizens process information in ways consistent with emotional attachments. Thus, strong partisans, Democratic or Republican, should better be able to make inferences about the candidates' policy positions. They simply "balance" their inferences with their own policy preferences and their partisan attachments (of the voluminous research here, see, e.g., Brent and Granberg 1982; Granberg and Brent 1974; Kinder 1978). To the extent that the world makes easy sense (i.e., Democrats liberal, Republicans conservative), this heuristic will aid learning.

Finally, consider emotion. Again, theoretically, we expect that the presence of threat in the environment will spur political learning while enthusiasm will not. An initial answer lies in Table 5, columns 1–2.[19] The estimation equations (each represented by a column) include a "lagged dependent variable"—the respondents' level of knowledge at the previous survey—to control for "regression to the mean" types of effects.[20] Substantively, three variables represent cognition: education (for capacity), campaign interest (for cognitive motivation),[21] and strength of partisanship (for the affect-heuristic model).[22] As much previous work predicts, education helps learning. The difference between a college-educated and a grade-school-educated citizen is .21 and .16 (for January–June and June–October, respectively), a substantial learning differential. Similarly, the difference in learning for the uninterested and the avidly interested is .06 and .14. The partisan-guided-learning hypothesis, however, fails. The strength-of-partisanship variable is statistically insignificant and in any case, it has the wrong sign (−.03 and −.05).

More to the point, examine the coefficients for emotional response. Our expectations are clearly met. In both sequences, a gain in knowledge is strongly associated with prior anxiety and not at all with prior enthusiasm. The gains associated with enthusiasm are minimal and statistically invisible. In power, anxiety measures up well against (though it does not dominate) the cognitive portion of the model. The difference in learning due to anxiety is about .12 (for both the early and late periods), or about the average amount of learning that took place in the campaign. The numbers are both statistically significant and substantively important. Further, the pattern is theoretically correct. Anxiety is positively associated with learning, and enthusiasm is not.[23] The dual-system model is again confirmed.

The duality of emotional response is made even clearer by turning our attention from political learning to political involvement, from citizens' acquiring new information to their engagement in the campaign. Our theory leads us to expect that for matters of already-learned behavior, for getting involved in an ongoing campaign, the key should lie in the positive-feedback mechanisms associated with enthusiasm rather than the attention-interrupt mechanisms of anxiety. Thus, the empirical pattern of the learning model in Table 5 should be reversed when we change our focus to the campaign involvement model.

Our theory predicts that involvement, measured by a change in campaign interest,[24] will vary as a function of changes in enthusiasm (while controlling

TABLE 5

Learning and Campaign Involvement as a Function of Emotion and Cognition during the 1980 Presidential Campaign

INDEPENDENT VARIABLES	LEARNING MODEL[a]		CAMPAIGN INVOLVEMENT MODEL[b]	
	JANUARY–JUNE	JUNE–OCTOBER	JANUARY–JUNE	JUNE–OCTOBER
Enthusiasm$_{(t-1)}$[c]	−.00	−.01	.08	.13*
	(.04)	(.04)	(.05)	(.04)
Anxiety$_{(t-1)}$	.12*	.12*	.06	.03
	(.05)	(.05)	(.05)	(.05)
Strength of partisanship$_{(t-1)}$	−.03	−.05	.15*	.08
	(.04)	(.04)	(.04)	(.04)
Education	.21*	.16*	.03	−.03
	(.04)	(.04)	(.04)	(.04)
Knowledge$_{(t-1)}$	−.42*	−.43*	.14*	−.11*
	(.04)	(.03)	(.04)	(.03)
Campaign interest$_{(t-1)}$	.06*	.14*	−.52*	−.50*
	(.03)	(.03)	(.03)	(.03)
Constant	.02	.03	.11*	.18*
	(.03)	(.04)	(.04)	(.04)
Number of cases	644	639	643	623
Root Mean Square Error	.27	.28	.29	.27
Adjusted R^2	.18	.19	.26	.27

Source: 1980 ANES Data.
Note: The entries are unstandardized regression coefficients with standard errors in parentheses.
[a] Learning is measured by the change in knowledge from one time to the next: [knowledge$_{(t)}$ − knowledge$_{(t-1)}$].
[b] Campaign involvement is measured by the change in campaign interest from one time to the next: [interest$_{(t)}$ − interest$_{(t-1)}$].
[c] For comparability, all variables are scored to a common range of 0–1. See Appendix.
*$p \leq .05$, two-tailed test.

for previous education, partisan intensity, and candidate knowledge). The expectation is confirmed. The empirical equations for the campaign involvement model are presented in Table 5. The key coefficients lie in Table 5, columns 3–4. During the spring primaries (January–June), the emotions are minimally—statistically insignificantly—associated with change in campaign involvement. If anything, partisanship is dominant. It is only during the fall campaign that candidate-induced emotional response spurs involvement. Crucially, the dominant factor becomes enthusiasm, not anxiety.

DISCUSSION

Our empirical work thus sustains a view that emotionality affects how people approach politics. Clearly, emotions are complex and subtle. Just as obviously, the simple valence model of political emotions can no longer stand. At the very least, mood states represent an amalgam of underlying feelings. Of this we are confident.

Our analyses also indicate that we gain theoretical leverage by turning to a dual-system model that produces complex emotions as a mixture of two distinct types: enthusiasm and anxiety. The first, associated with an ongoing emotional monitoring system, governs how far people allow themselves to engage with candidates and with politics more generally. The second, a manifestation of the behavioral inhibition system, spurs people to pay closer and more conscious attention to political matters and to act accordingly.

Our evidence carries weight because it confirms and extends an already-established theoretical view. We here rely on survey interviews about presidential candidates, a data source with well-known strengths and weaknesses. The data allow neither experimental control over the emotional stimuli nor subtle analyses of cause and effect. At best, we know the broad outlines and too little of the details or complexities. Yet we are able to show that a theory grounded in neurophysiology and in psychology can be usefully applied in the realm of politics. While we are in no way certain about the mechanisms that translate elementary processes (the stuff of neural transmitters, etc.) into political emotions and cognitions, we are now encouraged to think that further study will reward. Moreover, we *can* safely conclude that the emotional significance of information clearly affects what, when, and how we react.

In short, enthusiasm increases campaign involvement and anxiety enhances learning. Of course, matters are never so simple. Our data reveal subtle relationships among enthusiasm, anxiety, involvement, and learning. Nevertheless, we believe that the main story lies along these lines: when politics makes people anxious, people sharpen their eyes and pay careful attention; when politics drums up enthusiasm, people immerse themselves in the symbolic festival.

Understanding this enlarges our view of emotion's role in politics. We may be fairly sure that emotion matters not only in how it colors people's voting choices but also in how it affects the way they regard the electoral contest. This much is important enough. However, this new understanding has implications for how we, as social scientists, think about elections and political life.

First, finding that people's approach to politics depends on their emotional state tells us that the fundamental "voter" model should include a conditional component. That is to say, voters act differently under different conditions; they afford politics closer scrutiny when they are anxious than when they are enthusiastic. By introducing this conditionality, we can combine two views of citizen political involvement. The first divides the public by *stable trait*: active versus passive, attentive versus inattentive (classically, Converse 1962; Luskin 1987; Milbrath and Goel 1977; Neuman 1986; Verba and Nie 1972). The second view suggests that there are *variable states* that people can, at any given moment, fall into, say spectator versus participant (Marcus 1988a; Schattschneider 1960). We here propose a dynamic model of political learning that combines trait and state explanations to produce a richer view of how citizens inform their electoral choice. In states of anxiety, citizens activate their political consciousness; in states of enthusiasm, they engage their hearts in political affairs.

This emphasis on state-conditionality further points the way toward resolving a long-standing controversy about the basic character of citizen voting. Loosely speaking, a "public choice" school emphasizes the rational calculus of policy alternatives, while a "symbolic politics" school emphasizes the power of deeply ingrained normative commitments, such as partisanship, to shape voter preferences. The extent to which one or the other of these views characterizes voting is of obvious importance for democratic theory and has been the subject of years of intellectual debate and empirical investigation (e.g., Downs 1957; Enelow and Hinich 1984; Kinder and Kiewiet 1979; Markus and Converse 1979; Miller 1991; Miller et al. 1976; Rabinowitz and MacDonald 1989; Sears 1990; Sears, Hensler, and Speer 1979; Sears et al. 1980). While we do not hope to settle the matter, we believe that putting these "models" in competition may mislead.

Our understanding about anxiety and enthusiasm suggests that voters' emphasis on conscious rational choice (as opposed to long-standing commitment) will be conditioned on their emotional state. Voters can, and often will, vote their "standing decisions." However, they also rely on their internal emotional states to signal when to abandon their predispositions and begin conscious political choice. Emotionality thus empowers voters to confront their circumstances and react efficiently and appropriately. In the absence of anxiety, voters safely rely on preexisting partisan dispositions and the greater enthusiasm generated by the favored candidate; however, when disturbed by their emotional signals, voters pay more attention to the issues and no longer defer to established dispositions. Rather than being antagonistic or detrimental to citizenship, emotion enhances the ability of voters to perform their citizenly duties.

Because individual voters thus act differently under different conditions, we can expect that the quality of the entire electorate's behavior will vary when the macropolitical scene offers different blends of anxiety and enthusiasm. For example, consider conventional wisdom about positive and negative campaigns. Contemporary popular debate has almost universally condemned campaigns that seem to rely heavily on "attack" commercials while, implicitly, endorsing "positive" themes—odd. Our data indicate that positive campaigns, ones that emphasize visionary goals or candidate accomplishments, should do little for conscious deliberation. Instead, they seem best viewed as mobilization or activation—devices that yield a citizen involvement free from the burden of choice. On the other hand, campaigns that spur concern about the current state of affairs would seem much more likely to motivate people to pay closer attention to public affairs, to engage their full capacities, and to make rational decisions.[25]

More generally, the deliberative content of elections depends on the extent to which citizens feel comfortable or uneasy with the contemporary political situation. Partly, this comfort or discomfort will be a product of politicians' tactics. More interesting, though, is the likelihood that the public's emotional state will arise from social, economic, and political reality. Periods of economic depression (with the accompanying job losses and threats aimed at large numbers of families) will certainly activate people's emotional triggers and motive their political attention. Economic booms, on the other hand, may induce enthusiasm and, thus, political involvement without deliberation.[26] Similarly, failure during wartime should spur close attention while success should lead to grand parades in the collective fantasy. Because deliberation seems, at least in part, a function of emotionality, the nature of democratic government thus depends on how emotions get linked to political circumstances and how that link varies over time.

In the end, it appears that exploring the connection between emotions and political consciousness should yield much. We shall begin to appreciate how democracy handles changing social, economic, and political circumstances. At the very least, we shall begin to understand that the politics of emotion and rationality are closely intertwined.

APPENDIX

We wish to facilitate comparison between equations and to avoid the misinterpretations often associated with alternative scoring procedures. Because all the variables are measured by means of arbitrary survey-response scales, no natural metric suggests itself.

Retaining the survey-response scales (some running 0–1, 1–5, 1–7, or even 0–100) asks the reader mentally to translate all scale values before assessing the relative size of the regression coefficients. Mistaken inferences will arise when the variables' numerical scales differ by orders of magnitude, as do ours. On the other hand, standardizing coefficients by the population variances (using standardized regression coefficients) carries a more subtle threat to the reader's inferences. When different samples or subgroups form the focus of analysis and when they are marked by radically different variances (as in our case), the reader may be misled because coefficient values may be affected as much by the comparative variances as by the comparative impacts. For a commonsense discussion about scalar interpretations, see both the necessary warning by King (1986) and the entirely reasonable response by Luskin (1991).

We have chosen to standardize our numerical scales by the *range* of the variables—making all variables 0–1 scales. (For example, in the 1988 study, anxiety is measured as the mean of two 0–100 "thermometers." The values used here are simply the original values divided by 100.) Thus, our different measures are made *roughly comparable* to the 0–1 anxiety items of the 1980 surveys. With sensible caution, readers may safely distinguish "large" from "small" differences in the regression equations.

Notes

Some of the data utilized in this analysis were made available by the Inter-University Consortium for Political and Social Research. The Missouri data were made available through the good offices of the Public Policy Research Centers of the University of Missouri–St. Louis and Andrew Glassberg. (These surveys were conducted were conducted by the Public Policy Research Centers for MissouriNet, a private company.) We, of course, take full responsibility for our analyses and interpretations. We should like to thank Richard Brody, Russ Hanson, Roger Masters, W. Phillips Shively, W. Russell Neuman, Tim Cook, Betty Glad, Kathleen McGraw, James Kuklinski, John Mayer, Peter Salovey, Howard Eaton, David Watson, and Carolyn Lewis for their useful comments and suggestions.

1. There has been a spate of articles and commentaries in psychology debating the independence of affective and cognitive information-processing systems (see esp. Lazarus 1982, 1984; Zajonc 1980, 1982). The Western tradition has long presumed an antagonistic relationship between affect and cognition, or more aptly, passion and reason. Further, the tradition has presumed the necessity of encouraging reliance upon the latter and diminishing the role of the former on matters of public importance. We believe that this formulation is both misguided and misspecified. As we shall argue, affective processes work with cognitive processes in mutual support to enhance learning. Indeed, feeling states are themselves the product of extensive complex cognitive processes. Political scientists have tended to conflate cognition with conscious awareness in spite of substantial evidence that cognitive processing is not equivalent to conscious awareness (see Lewicki 1986).

2. In addition to altering mood, these systems have distinctive cognitive (Mayer and Gaschke 1988; Mayer et al. 1991) and distinctive physiological effects (Fowles 1980; Lazarus et al. 1965).

3. Aristotle recognized this distinction, warning the orator not to invoke a horror so great that it would immobilize the audience (*Rhetoric* 2.5.1383).

4. Jeffrey A. Gray. Conversation with Marcus, 9 April 1991.

5. Note that respondents who feel neither enthusiastic nor unenthusiastic, neither anxious nor calm, could opt for something in between—say, a score of 50. This is, as it turns out, the modal response. In the ANES wording, respondents who wanted to "skip the question" seem to have chosen a *no* response (rather than *yes*). Our questionnaire design had the desired effect of eliminating the "emotional arousal" contamination.

6. The sampling frames represent the Missouri electorate. The samples sizes for June, July, and October were 509, 509, and 502. Interviews were conducted by telephone. Other survey items reported, unless otherwise noted, were elicited by the standard ANES question format.

7. We earlier reported the results of an experiment conducted with these data. One half of each sample was presented with an anxiety-prominent version of the anxiety items and the other half of each sample was presented with a reassuring-prominent version of the anxiety items (i.e., anxiety was nominated as 100 or reassurance nominated as 100 for each half sample). The manipulation consisted of changing the order of presentation of each anxiety pair. The results demonstrated the importance of properly measuring the anxiety dimension (Marcus, MacKuen, and Glassberg 1989). The results we present are based on the half of each sample that responded to the anxiety-prominent measures of anxiety.

8. For example, for the June 1980 threat we sum the individual's June interview responses to *anger, disgust, afraid,* and *uneasy* (0–1 scores) and then divide by four. For July 1988, we sum *anxious* and *upset* (the 100-point thermometers) and divide by 200. This straightforward scale gives some weighting precision away to factor scoring but it retains the essential variance information across surveys.

9. Note that the public's sense of enthusiasm and anxiety appear to move in equal and opposite directions. The dynamics apparent in the aggregate data suggest that enthusiasm and anxiety might be opposite-signed versions of affect. Yet the internal evidence contradicts this view. If individuals are moved by events only along a single dimension, then those who increase their sense of anxiety should simultaneously decrease their sense of enthusiasm. To test this hypothesis, we may observe the correlation of change in enthusiasm and anxiety for the 1980 panel study. The observed correlations have the right sign (negative) but are of insignificant magnitude: −.06. Thus, while the aggregate reacts to news in a uniform manner, individuals do so in distinctive ways. For example, while the public as a whole might sour on a candidate, some individuals might increase their anxiety but not lose their enthusiasm and others might simultaneously become less enthusiastic but not necessarily anxious.

10. Bruce (1991) has done an extensive analysis of the structure of affect in the 1980 panel. He finds substantial changes over time: the correlations between the two dimensions range between near zero to the −.60s range. He finds no consistent patterns. This is as we would expect, given the unpredictable character of the changing circumstances confronting the body politic.

11. The specification of the analyses in Table 2 and Table 3 below do not include measures for issue proximity of candidate perceptions. Our previous work suggests that excluding issue-proximity measures is appropriate (Marcus 1988b). Candidate perceptions would be useful. However, our previous work discovered no interaction between candidate perceptions and the factors included here.

12. Presidential preference includes decided voters, leaners, and undecided (scored *Bush* = 0, *lean Bush* = .25, *undecided* = .50, *lean Dukakis* = .75, *Dukakis* = 1.0). Partisanship is measured by the standard seven-point scale from the Survey Research Center/Center for Political Studies (SRC/CPS) question sequence (scored *strong Republican* = 0, *weak Republican* = .166, . . . , *strong democrat* = 1.0). The "comparative" emotion measures calculate a score for Bush and Dukakis and take the (signed) difference.

13. Our discussion thus far has separated anxiety from enthusiasm as factors that distinctively stimulate attention

and engagement. We suspect that matters are not so simple. For example, uncertainty about a candidate's ability to guide the future will, naturally, lead to anxiety. We predict that this enhanced anxiety will move voters to gather information and more fully evaluate their electoral options. Yet we would not be entirely surprised if that uncertainty (and anxiety) also led risk-averse voters to lose enthusiasm for the candidate. Obviously, the clean theoretical distinction between anxiety and enthusiasm will be complicated in real life. Our finding that voters' candidate preferences (in the 1988 Missouri data) are independent of comparative anxiety overstates the case. Work with the 1980 and 1984 ANES studies suggests that anxiety's role is less than that of enthusiasm but does not disappear completely. Clearly, accepting anxiety's peculiar role in stimulating attention rather than merely diminishing enthusiasm, depends on a more direct test.

14. We exclude comparative anxiety in these equations because it, and its interaction, produce negligible coefficients. The simple model in Table 3 is subject to mistaken inference. Anxiety is theoretically and empirically linked to both partisanship and enthusiasm. In order to substantiate Table 3 we replicated the analysis using a "purged" version of anxiety rather than the raw score. To do so, we estimated auxiliary equations (regressing each candidate-specific anxiety term on the enthusiasm terms and partisanship) and retained the mean of the two "residuals" as a cleaner measure of anxiety. The replication produced results that were substantively similar and statistically crisper: Table 3 stands. In addition, we replicated the same experiments using a Bush–Dukakis dichotomy and estimated the equations in logistic form. Again, the theoretical inferences were unchanged.

15. A substantial body of studies in psychology have shown that negative events generate more cognitive activity than do positive events. For an excellent recent review, see Taylor 1991.

16. We are unable to proceed with our 1988 data because they were not collected in panel form (i.e., we did not interview the same respondents through the campaign). Thus, in order to test learning (the acquisition of knowledge), we need to turn to the 1980 data. Here we must use inferior measures of emotional response to take advantage of the panel design.

17. Survey respondents typically volunteer responses to prompts and, as the political campaign progresses, feel pressure to produce opinions about obviously important public figures. Further, respondents become willing to guess about personal qualities or about policies as they begin to learn anything at all about the candidate. Finally, even though genuine, such cognitions may be completely erroneous.

18. Here knowledge is tapped by the respondent's understanding that Reagan (more than Carter) wanted increases in defense spending, that Reagan (more than Carter) favored cuts in government spending on social welfare programs, that Carter (more than Reagan) was inclined toward détente with the Soviets, and (finally) that Reagan was more conservative than Carter. We choose these items because, in the aggregate, the public saw the candidates distinctively of the Left and of the Right. On the seven-point scales, the mean-mass perception of Reagan was a full point to the right of the mean-mass perception of Carter. (On other issues, the public was less certain that the candidates differed.) The knowledge measure is corrected for guessing. Assuming randomness, one would expect that there are an equal number of "correct" and "incorrect" answers. Thus, the measure assigns a score of 1 for those placing Reagan to the right of Carter, a score of −1 for those placing Reagan to the left of Carter, and a score of 0 otherwise.

19. For the individual-level analysis ahead, in Table 5, we have truncated individual below-zero scores to zero in order to retain a (0–1) "knowledge" scale. The problem of negative scores appears only with the individual analysis presented in Table 5, not in the aggregates presented in Table 4. Note that most of those not getting a "correct" score simply fail to place both candidates on the policy question. For the truncated scores, the summary knowledge means are .27 (January), .35 (June), and .42 (October).

20. We normally expect the coefficient associated with the "lagged dependent variable" to take on a large negative value—because deviations from the "mean" or "equilibrium value" will collapse toward that value. In the extreme, when any deviation from the overall mean disappears from one time to the next, the coefficient takes on the value of −1.0. On the other hand, we might expect some "positive feedback" in the sense that people who know more will learn more. Insofar as we fail to capture this phenomenon in the substantive variables elsewhere in the estimation equation, we expect a positive coefficient for the lagged dependent variable. The net (negative and positive) result will determine the observed value. In Table 5's equations, we find moderately large negative coefficients as is common for this sort of work. We draw no theoretical inferences from their estimates. More important, this "control" allows us to estimate more cleanly the independent effects of our theoretically interesting variables.

21. Here we are bending over backward by treating campaign interest as a cognitive factor. It is likely that campaign interest is itself a factor well imbued with a positive affect component (inasmuch as enthusiasm for political activity is a learned disposition and therefore likely under the influence of the behavioral approach system).

22. Strength of partisanship is measured by the standard SRC/CPS question sequence (scored *pure independent* = 0, *leaning independents and weak identifiers* = .5, *strong identifiers* = 1.0).

23. Note that we model the impact of emotional reactions on subsequent learning and involvement. We do not believe these relationships to be unidirectional. In fact, any common-sense understanding of politics suggests that as people learn more about the candidates and get more involved in the campaign, they will develop further emotional reactions. These expectations are sustained in the 1980 panel data. Table 6 models change in anxiety and enthusiasm as a function of previous candidate knowledge (knowing Reagan to be more conservative than Carter), level of involvement (campaign interest), and the lagged endogenous term and shows that the phenomena are dynamically interrelated.

Table 6. Emotional Response as a Function of Prior Learning and Involvement

INDEPENDENT VARIABLES	CHANGE IN ENTHUSIASM[a] JAN.–JUNE	CHANGE IN ENTHUSIASM[a] JUNE–OCT.	CHANGE IN ANXIETY[b] JAN.–JUNE	CHANGE IN ANXIETY[b] JUNE–OCT.
Knowledge (lag)[c]	.00	.05*	.10*	.07*
	(.02)	(.03)	(.02)	(.03)
Involvement (lag)	.01	.06*	.05*	.05*
	(.02)	(.03)	(.02)	(.03)
Enthusiasm (lag)	−.41*	−.39*	—	—
	(.03)	(.03)		
Anxiety (lag)	—	—	−.43*	−.35*
			(.04)	(.04)
Constant	.20*	.12*	.16*	.14*
	(.02)	(.03)	(.02)	(.02)
Adjusted R^2	.19	.18	.18	.13
Root Mean Squared Error	.21	.20	.20	.22

Source: 1980 ANES Data.

Note: Standard errors of the estimators are in parentheses.

[a] Change in enthusiasm is measured by the change in the enthusiasm score from one time to the next [$\text{enthusiasm}_{(t)} - \text{enthusiasm}(t-1)$].

[b] Change in anxiety is measured by the change in the anxiety score from one time to the next [$\text{anxiety}_{(t)} - \text{anxiety}_{(t-1)}$].

[c] For comparability, all variables are scored to a common range of 0–1. See Appendix.

*$p \leq 05$, two-tailed test.

The results suggest that people who become engaged in and learn more about politics find their emotional reactions reinvigorated. This mutual feedback system appears stronger during the June–October campaign period rather than during the January–June primary season, and for the anxiety–knowledge system rather than the enthusiasm–involvement system. However, the case is not settled. For example, if we control for the respondents' education and (the interviewers' assessment of) the respondents' January political information, the apparent mutual feedback disappears. Thus, changes in enthusiasm and anxiety seem related to permanent trait characteristics as well as (and perhaps rather than) temporary-state characteristics. Given these ambiguities, we must leave the details of the more complete system as an intriguing puzzle worthy of future work.

24. Campaign interest is measured by the standard ANES question: "Some people don't pay much attention to campaigns. How about you? Would you say that you are very much interested, somewhat interested, or not much interested in following the political campaigns this year?" The aggregate level of interest remained roughly constant during the 1980 primary and general election campaigns. In modeling the change in "involvement," we are modeling the factors that sustained people's interest in the campaign. Note that campaign interest is distinct from knowledge. Looking at the 1980 study, we find that self-reported campaign interest is highly correlated with the respondents' reported actions of reading of public affairs in their newspaper and watching the national news on television. Knowledge is more closely associated with their education and the interviewer's assessment of the respondents' political information.

25. The term *negative campaign* is used to characterize elections that are dominated by "attack" commercials. However, the term is used without great precision. Is a negative campaign distinguished from other campaigns by the proportion of the negative versus positive material produced by either or both candidates' campaign staffs? Does material become negative when it compares the candidates' behavior against some ideal standard or need it employ tasteless or pejorative language? Or is a negative campaign one in which attack materials, of whatever proportion and of whatever character, have had apparently great impact? Our appreciation for the potential benefits of "negative" campaigning will depend on our obtaining a firmer grasp on the phenomenon itself. In any case, we do not wish to make a normative judgment on the virtue of different sorts of emotional campaigns until we know more.

26. Evidence on this matter is relatively weak. Nevertheless, our speculation is sustained in Nie, Verba, and Petrocik's (1976) historical survey data on the ability of people to translate class-oriented preferences into presidential choices. During the anxiety-provoking thirties and sixties, the correlation between class-related issue preferences and voting was relatively strong, while during the relatively calm fifties and seventies, that correspondence weakened considerably.

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WOMEN'S ASSOCIATIONS AND THE ENACTMENT OF MOTHERS' PENSIONS IN THE UNITED STATES

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Mothers' pensions were the first explicit welfare benefits established outside of poor relief in the United States. Contrary to established wisdom in political science, their enabling statutes spread very quickly across most states in the 1910s, with smaller, nonindustrial states often in the vanguard. Previous research concerning the predictors of state-level policy innovations has focused on a small subset of possible explanatory variables, typically economic or electoral conditions. We operationalize and test hypotheses about the influence of economic conditions, culture and ideology, electoral politics, governmental institutions and prior public policies, and the role of business, labor, and women's voluntary groups on the priority of state enactments. Our findings indicate that widespread federations of women's voluntary groups exerted a powerful influence on mothers' pension enactments even before most American women had the right to vote. We demonstrate the value to empirical political science of theories and variables referring to gender and women's politics.

Received wisdom tells us that America's modern welfare state did not start until the Social Security Act of 1935. Although there are many problems with this view (see Skocpol 1992), its most obvious omission has to do with "mothers' pensions," the forerunner of the Aid to Dependent Children portion of Social Security and the "grandmother" of Aid to Families with Dependent Children. Forty states passed mothers' pension laws by 1920; four more, prior to the onset of the Great Depression in 1929; and two more, in the early 1930s. Mothers' pensions (sometimes called "widows' pensions") were state-level enabling statutes authorizing local governmental authorities to make regular payments directly to impoverished mothers (and occasionally other caretakers) of dependent children. Benefit payments were meant to cover at least part of the cost of raising such children in their own homes, so they would not have to be placed in foster homes or institutions such as orphanages. Mothers' pensions were the first explicit welfare benefits established outside of poor relief in the United States.

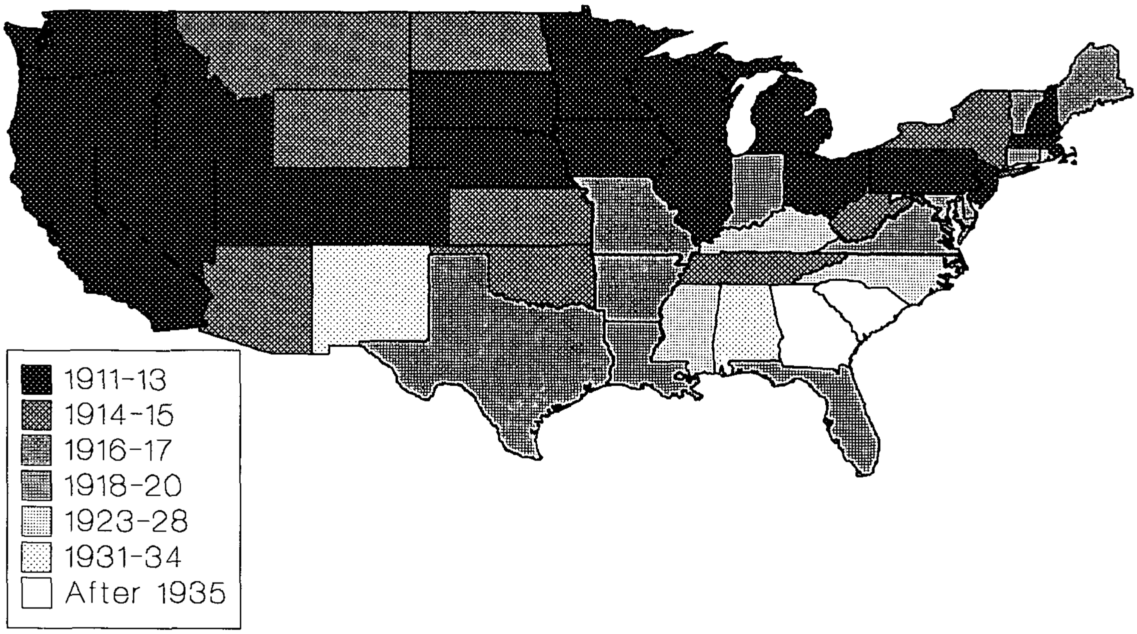
Political scientists have special reason to be curious about the origins of mothers' pensions, because the pattern of their enactment does *not* conform to established disciplinary wisdom about the spread of legislative innovations across the American states. Jack Walker (1969, 1971) created an "innovation score" for the states based on how quickly, on average, each adopted 88 different programs between the 1800s and 1965. Using this composite score, Walker argued that wealthier, more industrialized, and more urban states were usually the first to enact new programs. He maintained that the Rocky Mountain states, despite high literacy scores for their populations, were "among the slowest to adopt new programs" (1971, 359). But (as we shall see) urbanization and industrialization do not predict the relative priority of state enactments of mothers' pensions, while more literate states tended to enact these laws earlier. Fourteen states, including Rocky Mountain and other small, nonindustrial states, enacted mothers' pensions much sooner than Walker's composite score would predict: Nevada, South Dakota, Idaho, Iowa, Illinois, Nebraska, Arizona, Utah, Wyoming, Oklahoma, New Hampshire, Montana, West Virginia, and Tennessee.

Walker's approach to explaining legislative innovations was criticized by Gray (1973). She stressed wealth and electoral competitiveness as the factors differentiating the most innovative states from others and offered a "user interaction" model to explain why programs typically diffused across the states according to the S-shape of a cumulative normal curve. This model posits a gradual learning process across the states. But Gray acknowledged that mothers' pensions were an "aberrant" case for her user interaction model. After the first adoption by Illinois in 1911, 18 states adopted mothers' pension laws in 1913 (see Appendix). Both Walker and Gray agree that mothers' pensions spread across the states much more quickly than legislative innovations usually do. During the early twentieth century, it normally took two decades for the first 20 states to adopt a legislative innovation (Walker 1971, 377); but 40 states enacted mothers' pensions in less than one decade after 1910.

Why were mothers' pensions enacted so quickly across most of the states? Why did some states, including many smaller, non-industrial states, pass these statutes sooner than others? We shall present and empirically test potential explanatory arguments from a variety of scholarly literatures. A distinctive contribution of our analysis is the wide range of arguments under consideration. Some of these stress cultural and ideological influences, while others focus on business, organized labor, and the social composition of the work force. Still others hypothesize that

FIGURE 1

Mothers' Pension Laws: Dates of Enactment



the enactment of mothers' pensions may have been affected by electoral conditions or by governmental capacities and prior public policies about schooling and child labor. In addition, we introduce and test arguments about the importance of nationwide federations of women's voluntary groups in agitating for mothers' pension legislation during the 1910s. This type of variable has not previously been included in quantitative analyses of welfare state formation or policy innovation across the states, but it proves to be very significant. We show that even before most American women had the right to vote, they were able to work through voluntary associations to set agendas of public debate over social welfare, persuading state legislatures to enact the first explicit authorizations for social spending for mothers and children.

Operationalizing the Dependent Variable

In studying the relative priority of state enactments of mothers' pension statutes, we have examined a number of different operationalizations of the pattern. These operationalizations include a dichotomous split between early enactors by 1913 and all later enactors like Gray's dependent variable (1973, 1182–83), a split between pre-1915 and post-1915 enactors, and a continuous measure of the number of years prior to 1935 that each state first passed mothers' pensions. In the end, however, we settled on a seven-part ordinal scale. The values are illustrated in Figure 1. The period 1911–13 equals 6, 1914–15 equals 5, etc. down to 0 for "after 1935.")

Our ordinal scale groups state-level enactments into meaningful temporal clusters.[1] A single chronological year of time simply does not have the same meaning throughout the 1911–35 period. During the 1910s, events having to do with social reforms moved very rapidly, and most state legislatures met on alternate years. It made a difference whether a state enacted mothers' pensions in (or before) 1913 (during the first major wave of enactments) or in the successive waves that peaked in 1915 and 1917. Thus, these waves are coded as three separate categories, even though they are close together in linear time. The next category includes states that enacted laws toward the end of, and right after, World War I, from 1918 to 1920. And the next cluster groups four states that legislated in the mid-1920s, a period that generally saw very little reform activity compared to the 1910s. Finally, we split the 1930s into two categories even though merely four laggard states are involved, because two of them (New Mexico and Alabama) enacted laws before the Social Security Act of 1935 established federal subsidies for mothers' pensions, while the other two (Georgia and South Carolina) waited until after this national incentive was created.

EXPLANATIONS FOR THE ENACTMENT OF MOTHERS' PENSIONS

We shall introduce major types of arguments that have been or might be used to explain the relative timing of mothers' pension enactments and describe the independent variables specified to operationalize these arguments. Constraints on available data made it impossible for us to use time series analysis for this study. Often, data are available only at ten-year intervals; thus, many of our independent variables are measured at, or close to, the year 1910, right at the beginning of the period during which mothers' pensions diffused across the states. Still, wherever possible, we developed independent variables that used information for several time points during the 1910s or, better yet, that embodied ordinal measures of temporal priority during the 1910s and 1920s. Fortunately, our most important independent variable—endorsements of mothers' pensions by women's groups in the 48 states—measures temporal priority, as well as the presence or absence of endorsements.

Culture and Ideology: New Ideas about Home Life and Mother Care

Historical accounts routinely point to the 1909 White House Conference on the Care of Dependent Children as the event that launched the diffusion of mothers' pensions across the states (see, e.g., Gray 1973, 1183; Leff 1973, 399; Lubove 1986, 97–98; Vandepol 1982, 230). By the early twentieth century, the argument goes, social reformers had come to believe that home care and mother love were better for children than any kind of institutional care (Ladd-Taylor 1987; Vandepol 1982). The conviction spread that orphans should be placed in foster care, rather than in orphanages, and that the children of impoverished widows should be kept at home, rather than (as before) separated from their mothers and placed in institutions. The 1909 White House conference is said to have crystallized this new valuation of home life among many settlement workers and other professional social reformers, although the relevant conference resolution (see U.S., Senate 1909, 9–10) did not call for *public* mothers' pensions. Indeed, historians agree that private charity organizations resisted public mothers' pensions as a way to operationalize the new consensus about home life, because these bodies had a stake in raising private money to aid "worthy widows" (Drew 1987, chap. 5; Lubove 1986, 101–6). Leading charity officials argued that widows and their children should be aided by kin and private charity, not by governmental benefits (see Devine 1913; Richmond 1913).

Arguments about the activities of professional reformers and charity groups are difficult to operationalize quantitatively. In the early stages of this study, we considered a range of possible variables. We examined the per capita number of delegates that each state had in attendance at the 1909 White House conference, reasoning that perhaps participants in the conference carried word of the new ideological enthusiasm about home life back to their states. More broadly, we considered possible independent variables referring to the relative numbers in each state of social settlements, settlement residents, Consumers' Leagues, and affiliates of the National Child Labor Committee (an organization that crusaded specifically for laws to help prevent child labor; see Chambers 1963, chap. 2; Trattner 1970). We likewise considered measures of the organizational presence of Charity Organization Societies, which were the most active associations of private charity workers *opposed* to public social spending for measures such as mothers' pensions (Katz 1986, chap. 3). But as we examined variables referring to advocacy groups that might have either furthered or delayed the enactment of mothers' pensions, it became apparent that such measures were not working in the statistical analysis, for a good substantive reason: both charity associations and associations of progressive reformers tended to be concentrated in major northeastern and midwestern cities, and most delegates to the 1909 White House conference came from the same cities. Although historians of U.S. social welfare may be correct in arguing that ideological arguments for or against mothers' pensions were articulated most forcefully by groups concentrated in New York, Massachusetts, Illinois, and other urban–industrial states, these groups were not the ones responsible for spreading (or delaying) legislative enactments across all 48 states. Indeed, had urban reformers been the only ones involved in advocacy for mothers' pensions, it is doubtful that these laws ever would have spread across the nation during the 1910s.

The activities of elite advocacy groups aside, home life and motherhood were values celebrated and reinforced by the national mass circulation women's magazines that reached millions of middle-class readers during the early 1900s (Waller 1987; Waller-Zuckerman 1989). These magazines were successfully distributed across the entire United States, including to many small towns and farms; and during the early 1900s, their editors often sponsored campaigns for social reform causes. One of the "big six" women's magazines around 1910 was the *Delineator,* a magazine of fashion, literature, and civic affairs put out by the maker of Butterick fashions. This magazine was sold both through the mail to subscribers and in stores across the country alongside sewing patterns for ladies' dresses. The *Delineator* had a circulation of 930,600 in 1912, when it was the third-largest women's magazine (Waller-Zuckerman 1989, 737, table 2). During 1912 and 1913, the *Delineator* advocated mothers' pensions in a series of emotionally engaging and richly illustrated feature articles (for full details, see Skocpol 1992, 432–42). Articles advocating mothers' pensions also appeared in *Good Housekeeping* and *Collier's,* as well as in the Scripps–McCrae and Hearst chains of newspapers (Bullock 1915, xviii; Leff 1973, 406).

We include in our statistical analysis two variables that may measure cultural and ideological circumstances facilitating mothers' pensions. So far, we have not been able to find usable state-by-state circulation data for the early 1910s for the *Delineator* (or other magazines that promoted mothers' pensions); but we do have some state-by-state data for 1920. Using this imperfect data as a rough indication of what earlier circulation might have been, the variable DELINEATOR identifies the per capita number of subscribers per state. In addition, LITERACY measures the proportion of each state's population over 10 years of age that could read and write in 1910. A more literate population might have been more receptive to the spread of new ideas, including the proposals for mothers' pensions advocated in the print mass media.

Modernization: Industrialization and Urbanization

Many political scientists have argued that the industrialized, urban, and wealthier states enact social policies sooner and fund programs more generously (e.g., Dawson and Robinson 1963; Dye 1966; Gray 1973; Hofferbert 1966; Walter 1969).[2] At the same time, students of cross-national patterns of social policy development have invoked industrialization and urbanization as key explanatory variables, arguing that these socioeconomic transformations render families more vulnerable to interruptions of wage income and simultaneously create new wealth that enables governments to address ever-more-visible social needs (Cutright 1965; Jackman 1975; Kerr et al. 1964; Wilensky and Lebeaux 1958). Although modernization arguments have been found *not* to apply to the origins of old-age pensions or social insurance in the United States as contrasted to Europe (Collier and Messick 1975; Flora and Heidenheimer 1981; Orloff and Skocpol 1984), they nevertheless might apply to the intra-U.S. origins of *mothers' pensions,* which were intended largely for the "worthy widows" and children of deceased male breadwinners. During the original campaigns for new legislation, mothers' pensions were projected to cost taxpayers no more than did existing public orphanages and outdoor poor relief (Vandepol 1982, 230). Still, supporters of modernization theories can maintain that industrial and urban states would have experienced the social problems to be addressed by mothers' pensions sooner and more acutely. Or they might argue, along with Walker (1969, 1971), that more developed, wealthier states would have had the extra resources to allow their officials to be more cosmopolitan and innovative in picking up on new kinds of legislation.

Certain cross-state and cross-national literatures thus lead us to expect positive effects on early legislative enactments from MANUFACTURING (per capita value of states' manufacturing products) and URBANIZATION (percentage of each state's population living in cities of twenty-five thousand or more). Because of inadequate data for this time period, we do not have a valid measure of overall state economic wealth.[3] Nevertheless, one of the variables defined below, STATE GOVERNMENT REVENUES, could be construed as an indirect indicator of state wealth.

Business, Labor, and Work Force Composition

In recent years, most comparativists studying Western welfare states have argued that strong working-class organizations have been the primary forces behind the enactment and expansion of public social benefits. According to these *working-class-strength* theories, the relative size of trade unions and control of government by working-class parties are key variables to explain the early enactment and fiscal generosity of social policies—which are understood to replace market income for wage earners and their families (Korpi 1980, 1983; Shalev 1983; Stephens 1979). Comparativists of this persuasion treat business interests and business organizations as opponents of the establishment and expansion of social policies.

Some scholars have suggested that U.S. businesses and other conservative interests did little to oppose mothers' pensions because the state-level enabling statutes did not include spending commitments or new taxes (Davis 1930, 581–82; Leff 1973, 404–5). Decisions about spending for mothers' pensions were left in the hands of local authorities. On the other hand, Drew argues that textile manufacturers did oppose mothers' pensions, because their businesses were major employers of female and child labor, and they therefore wanted to keep widows and their children in the labor force (1987, chap. 6). Comparativists who emphasize working-class strength have not specifically discussed U.S. mothers' pensions. Nevertheless, we do know that the major U.S. union federation of the day, the American Federation of Labor (AFL), endorsed mothers' pensions at its 1911 national convention (American Federation of Labor 1911, 357–58). Perhaps mothers' pensions fit the pattern hypothesized by working-class-strength theorists, passing first in states with many unionized wage earners and well-established trade union federations.

Given the paucity of relevant state-level statistics in this era, our explanatory variables referring to business and labor are less than ideal. The general variable MANUFACTURING introduced above could be construed as an indicator of business interests that might have opposed mothers' pensions in the various states. We also have two broader measures of socioeconomic contexts in which businesses (and other actors) might have been reluctant to see mothers' pensions enacted. These are PERCENT CHILD LABORERS (referring to the percentage of manufacturing workers who were less than age 16 in 1910) and PERCENT WORKFORCE FEMALE (referring to the percent of total gainful work force 10 years and older in 1910 who were females). In states where many women or children were employed, business owners may have opposed mothers' pensions, which were intended to keep widows at home and their children in school; and other groups may not have felt a such a pressing need to find new sources of income for widows and their children.

Before 1939, there are no state-level statistics on numbers of unionized wage earners. (Nor is it feasible to construct such statistics out of national AFL data because the constituent units of the AFL were occupationally defined unions, not state or local organizations.) We have therefore addressed the question of organized labor's possible involvement in lobbying for mothers' pensions in a different way. Labor historians have established that state-level Federations of Labor in this period often concentrated on legislative lobbying (Fink 1973; Taft 1968). Older state federations were, arguably, more likely to have built up effective lobbying organizations and political alliances, both of which could have been used to push for mothers' pension laws during the 1910s. Our variable AGE OF STATE FEDERATION thus measures the number of years that each state labor body had been in existence as of 1912, the first full year in which state federations could have implemented the 1911 national AFL endorsement of mothers' pensions.

Electoral and Partisan Contexts

Previous research suggests that mothers' pensions "were not a central party concern [and] rarely even merited mention in party platforms of states that enacted such laws. . . . Some gubernatorial messages dealt with mothers' pensions, and some state legislators discussed their advocacy of pensions, but on the whole, politicians remained followers rather than molders of public opinion" (Leff 1973, 405–6). Mothers' pension laws passed by overwhelming bipartisan majorities in state legislatures and referenda (pp. 400–401). Their advocates "worked hard to keep the issue from becoming a party issue" by consciously invoking bipartisan themes of motherhood, efficiency, and justice (Davis 1930, 581). Yet even if mothers' pensions were not normally promoted by political parties or enacted through partisan conflicts, patterns of electoral participation and balances of power among political parties may nevertheless have created *contexts* that facilitated or impeded the enactment of these laws during the 1910s and 1920s. We explore this possibility in several ways.

Many political scientists believe that interparty electoral competition is the most important explanation for state-level variation in social policy innovation and spending (Cnudde and McCrone 1969; Key 1949, 1956; Lockard 1959; Sharkansky and Hofferbert 1969). They argue that greater competition induces politicians to court mass support, especially when voter turnout is high. Our index of PARTY COMPETITION represents the average level of competition for state offices between 1910 and 1916; and we use a VOTER PARTICIPATION variable for each state that measures the average voter turnout in the presidential elections of 1908, 1912, 1916, and 1920.[4]

Some cross-national researchers have argued that "leftist" parties in control of government tend to enact new social policies because they are allied with labor and other popular groups representing potential beneficiaries (Esping-Andersen 1985; Shalev 1983; Stephens 1979). But for other cross-national researchers, the strength of "rightist" parties matters more, because they may block or delay new or expanded social policies (Castles 1982; Wilensky 1981). Obviously, it is not easy to apply "left" and "right" labels to U.S. party politics in the period of interest here. Arguably, AVERAGE DEMOCRATIC PARTY STRENGTH from 1910 to 1916 is an indicator of "right party" strength on mothers' pensions, because Democrats in this period tended to enroll support from southerners and Catholics, who might have been skeptical of this kind of enhanced governmental power. We hypothesize that this variable might have a negative relationship to early mothers' pensions enactments, regardless of region.

It makes little sense to argue that average Republican party strength is a measure of "left" influence during this period. The decade of the 1910s, however, was a time of challenges from third parties, such as Progressives, Socialists, and Prohibitionists. These parties tended to ally with reformers and with either labor or organized women, all of whom wanted greater governmental involvement in social welfare and social regulation. Republicans were often influenced to move in reformist directions in states where one or more of these third parties was strong. Thus, we test for positive effects on the enactment of mothers' pensions of AVERAGE REFORM VOTE, defined as the average percentage of presidential votes cast for the Progressive party in 1912; the Socialist party in 1912, 1916, and 1920; the Prohibition party in 1916; and the Farmer–Labor party in 1920.[5]

Electoral considerations aside, the varied rules of the game in state-level politics may have influenced the relative timing of the enactment of such reforms as mothers' pensions. The early 1900s was an era when party machines were being criticized by progressive reformers advocating new electoral routines such as the referendum, recall, and initiative (McCormick 1986, pt. 3; Shefter 1978). Although referenda were rarely used to enact mothers' pensions, states that had established some or all of these reforms might have had a "progressive political climate" amenable to other reforms, including mothers' pensions. Our variable ELECTORAL REFORMS measures the average number of years before 1930 that each state enacted these three measures.[6]

A final transformation of U.S. electoral politics in the early twentieth century might well have been more consequential than any other for the enactment of mothers' pensions (and other social policies targeted on women and children): campaigns for women's suffrage gained momentum after 1900, many states granted women the right to vote during the 1910s, and national women's suffrage was finally ensured by constitutional amendment in 1920. In the words of historian Mark Leff: "Although the more militant wing of the suffrage movement feared that mothers' pensions might damage the cause of sexual equality by glorifying the woman's place at home, supporters of women's suffrage generally favored mothers' pensions. Moreover, the legislatures that most easily and quickly approved mothers' pensions were usually those actively considering women's suffrage or chosen by a sexually unrestricted electorate" (1973, 407–8). This statement echoes the view of suffragists themselves that female voting spurred states to enact better laws for women and children (see Porritt 1917). We test Leff's arguments by examining the effect on the timing of mothers' pension enactments of FEMALE SUFFRAGE BEFORE 1920 (i.e., all states that granted women the right to vote in state elections prior to the national constitutional amendment of 1920).

Women's Voluntary Groups

Social scientists who study "politics," especially with quantitative indicators, usually mean electoral politics. But social historians have demonstrated that this narrow definition of politics overlooks critical kinds of female political participation and effectiveness. In a pathbreaking article, Paula Baker (1984) argued that American women, who were mostly excluded from voting during a century when all white men enjoyed the suffrage (1820–1920), instead developed voluntary organizations to extend the moral values of domesticity into civic affairs, expressing special concern for conditions influencing mothers, children, and families. Higher proportions of women in the United States than in any other nation in the nineteenth and early twentieth centuries achieved college and postgraduate levels of education, a situation that facilitated female civic influence in local communities and in state and national policy debates (Sklar 1993; Skocpol 1992, chap. 6; Skocpol and Ritter 1991). By the early twentieth century, higher-educated, mostly single women were leaders in hundreds of U.S. social settlements and in staff-led groups like the National Consumers' League that advocated new social welfare policies (Davis 1967). Arguably even more important, across the entire nation by 1900, middle-class and elite married women had developed *federations* of voluntary associations that became involved in shaping public opinion and lobbying legislators for new social policies. The major women's federations enjoyed a weighty organizational presence simultaneously at the national, state, and local levels. They could set priorities and rapidly communicate ideas across virtually the entire United States.

Decades ago, Ada Davis argued that women's voluntary groups crucially influenced the enactment of mothers' pensions with such nonpartisan and nonelectoral methods of politics (1930, 582–84). As Leff notes, major associations of middle-class married women were active proponents of mothers' pensions (1973, 408–9). Specifically, the National Congress of Mothers, with over fifty thousand dues-paying members in 22 state branches, endorsed mothers' pensions at its 1911 convention (National Congress of Mothers 1911, 196; National Congress of Parents and Teachers 1947, 199; G. Robertson 1912); and the Congress's national and state leaders continued to push legislatures to enact and improve mothers' pensions throughout the 1910s (Skocpol 1992, 445–56 and table 9). Meanwhile, the million-member General Federation of Women's Clubs, with state federations and local clubs in every state of the union, endorsed mothers' pensions at its 1912 biennial convention; and many state federations campaigned for these laws between 1912 and 1916 (General Federation of Women's Clubs 1912, 600; Skocpol 1992, 442–45 and table 9; Wood 1912, 353).

Skocpol argues that widespread federations of locally organized groups have always had a political "comparative advantage" in U.S. social politics and that this was especially the case during the early twentieth century (1992, pt. 3). The arenas for enacting any social legislation during this period were the 48 states, which presented enormous problems of coordination for reformers. Furthermore, the major U.S. political parties of the day were often not effective programmatic promoters of new legislation. Groups like the women's federations that could create sudden, nationwide groundswells of public opinion in favor of particular kinds of legislative reforms (and at the same time had an organizational presence in many local legislative districts) could help to bring about the rapid waves of state-level enactments that characterized the most successful social policy innovations of the Progressive Era. Such rapid diffusion of enactments across the states in turn helped to counter the arguments typically made by conservatives that new regulations or higher public expenditures would hurt their state's relative economic competitiveness (see Graebner 1977; D. Robertson 1989).

For the first time in quantitative research on U.S. politics, we have developed variables to explore the impact of women's voluntary associations on the enactment of state-level social legislation. DENSITY OF WOMEN'S CLUBS, based on records about the local units in the General Federation of Women's Clubs, refers to the number of local women's clubs per capita in each state in 1910. In addition, we have developed a measure of the reported *efforts* by women's groups on behalf of mothers' pensions. Along with scattered information from other sources, we have systematically surveyed reports by the various state federations to national biennial conventions of the General Federation of Women's Clubs in 1912, 1914, 1916, 1918 and later, as well as the state branch reports to the National Congress of Mothers that were regularly reprinted in *Child-Welfare Magazine* during the 1910s and early 1920s. The variable WOMEN'S GROUP ENDORSEMENTS tells us whether, and how soon, one or more women's associations in each state reportedly endorsed mothers' pension legislation.[7] (The dates used to form this variable appear in the Appendix; it ranges from 4 for states where women's groups endorsed mothers' pensions in 1912–13 to 0 for states where there is no evidence of any endorsement.) Clearly, if the scholars who argue that women's voluntary organizational capacities and lobbying activities on behalf of mothers' pensions made a telling difference are correct, then the states where official endorsements occurred and where those endorsements came earlier should have enacted mothers' pensions sooner.

Governmental Capacities and Related Public Policies

In recent years, certain analysts have moved away from seeing public policies simply as responses to socioeconomic conditions and the demands of social groups. "Bringing the state back in" to explanations of policymaking, they have argued that the administrative and fiscal *capacities* of governments may affect the willingness of officials, politicians, and social groups to envisage and support new policies that would use, and extend, government power (see Skocpol 1985). Similarly, Hugh Heclo (1974) has argued that new policies often arise through processes of "political learning," in which officials and other political actors reflect upon the strengths and weaknesses of existing policies and seek to build upon earlier policies, correcting for problems revealed by their operation. Preexisting policies, the institutional arrangements of governments, and the organizational characteristics of systems of political parties may also influence the goals, access, and alliances of officials and social groups active in politics (Amenta and Zylan 1991; Orloff and Skocpol 1984; Weir and Skocpol 1985).

We have devised variables to test a variety of state-centered hypotheses. STATE GOVERNMENT REVENUES indicates government revenues per capita as of 1913. Perhaps fiscally wealthier states enacted mothers' pensions first, as some of the political science literature on legislative innovations would lead us to expect (see Walker 1969, 1971). We also examine per capita STATE EXPENDITURES ON CHARITIES, HOSPITALS, AND CORRECTIONS. Advocates of mothers' pensions argued that these laws would save money otherwise spent on charity, so that states already spending heavily in this area might have been more prone to enact mothers' pensions. Another variable, GOVERNMENT OFFICIALS (as a percent of state population) indicates overall governmental administrative capacities that might have increased proclivities by either officials or social groups to support a new *public* welfare benefit.

In addition to examining the foregoing general kinds of "state capacity," we use variables referring to governmental capacities and preexisting public policies closely related to mothers' pensions. Historians have noted that juvenile court judges were early advocates of mothers' pensions in some states (see Lubove 1986, 99–100; Tiffin 1982, 122–23). What is more, Leff, Orloff, and Skocpol argue that the prior existence of juvenile courts as new, professionally run bodies established by progressive reformers reassured elite and middle-class Americans (who were generally worried about "political corruption" in the 1910s) that mothers' pensions could be administered honestly and effectively (Leff 1973, 401; Orloff and Skocpol 1984, 745). Finally, John Drew (1987) has argued that pressures for the enactment of mothers' pensions came from prior success in efforts to enact child welfare reforms, including laws prohibiting child labor, laws promoting public schooling, and laws establishing juvenile courts as agencies to deal with family problems and child misbehavior. Drew reasons that mothers' pensions "completed" this set of reforms by allowing juvenile authorities to keep fatherless children at home with their mothers, at school, and out of wage work.

We measure the AGE OF JUVENILE COURTS, the number of years each state's juvenile courts (if any) had been in existence by 1910; and our dichotomous variable CHILD LABOR LAW BY 1889? indicates those states that enacted child labor laws especially early. In addition, we draw on Ogburn (1912) for two indicators of the rigor of state-level child labor laws by 1909: AGE LIMIT for child labor in manufacturing and the LEVEL OF INSPECTION responsibility assumed by state agencies charged with enforcing child labor laws. According to Drew's (1987) argument, all variables referring to juvenile courts and child labor laws and enforcement should have positive relationships to early enactments of mothers' pensions. Drew would also expect per capita STATE EXPENDITURES ON PUBLIC SCHOOLS to be positively related to mothers' pensions, while PERCENT CHILD LABORERS (the percentage of manufacturing workers less than 16-years-old in 1910) would be negatively related to early enactments.

Finally, we consider one other variable in line with Drew's ideas. EXEMPTIONS? is a dichotomous variable indicating whether states exempted from the enforcement of their child labor laws orphans, children of widows, or children of disabled parents. If states did have such exemptions, then, arguably, widows could rely for their livelihoods on child labor; if not, then, presumably, mothers' pensions would have been more necessary to sustain widows and their nonworking children.

ANALYZING ALTERNATIVE ARGUMENTS ABOUT THE ORIGINS OF MOTHERS' PENSIONS

We started the analysis of our data by constructing separate regression models, one for each category of explanatory hypotheses introduced. Table 1 lists the independent variables that we entered into the regression equations for each type of explanatory argument, and gives the statistical results for each cluster of variables.[8] Within Table 1, Models 1, 2, 3, 5, 7, and 8 present results for regressions that included all of the variables within each category of explanatory argument. Models 4, 6, and 9 offer pared-down versions of models 3, 5, and 8, respectively. At this stage of discussion, we are not pitting variables from the different explanatory approaches against one another; that part of our analysis comes later. First, it is instructive to discuss what we can learn about the efficacy of each kind of argument considered apart from the others.

Contrary to much received wisdom in political science, the modernization variables we tested (both jointly and singly) did *not* end up predicting the priority of mothers' pension enactments across the states. As displayed in model 2, these negative findings about INDUSTRIALIZATION and URBANIZATION run directly counter to Walker's (1969, 1971) generalizations about "innovation in state politics." Also

TABLE 1

Models for Alternative Explanations of the Timing of Mothers' Pension Enactments

VARIABLES	MODEL 1	MODEL 2	MODEL 3	MODEL 4	MODEL 5	MODEL 6	MODEL 7	MODEL 8	MODEL 9
Culture and Ideology									
Literacy	.151**	—	—	—	—	—	—	—	—
	(.027)								
Delineator	.001*	—	—	—	—	—	—	—	—
	(.001)								
Modernization									
Manufacturing	—	.002	—	—	—	—	—	—	—
		(.003)							
Urbanization	—	.015	—	—	—	—	—	—	—
		(.023)							
Business, Labor, and Workforce Composition									
Percent child laborers (−)	—	—	−.183	—	—	—	—	—	—
			(.109)						
Age of state federation	—	—	.046*	.055**	—	—	—	—	—
			(.018)	(.018)					
Percent Workforce female (−)	—	—	−.121**	−.169**	—	—	—	—	—
			(.040)	(.028)					
Electoral and Partisan Contexts									
Voter Participation	—	—	—	—	−.001	—	—	—	—
					(.019)				
Average democratic party strength (−)	—	—	—	—	−1.932	—	—	—	—
					(1.253)				
Electoral reforms	—	—	—	—	.033	—	—	—	—
					(.030)	—	—	—	—
Female suffrage before 1920	—	—	—	—	.078	—	—	—	—
					(.306)				
Party competition (−)	—	—	—	—	−4.076	−4.432**	—	—	—
					(2.613)	(1.466)			
Average reform vote	—	—	—	—	8.099	18.417**	—	—	—
					(5.439)	(3.696)			
Women's voluntary groups									
Density of women's clubs	—	—	—	—	—	—	.088*	—	—
							(.034)		
Women's group endorsements	—	—	—	—	—	—	.758**	—	—
							(.098)		
Governmental Capacities and Related Public Policies									
State government Revenues	—	—	—	—	—	—	—	.023	—
								(.053)	—
State expenditures on charities, hospitals, and corrections	—	—	—	—	—	—	—	−.055	—
								(.703)	—
Government officials	—	—	—	—	—	—	—	.095	—
								(.229)	—
Age of juvenile courts	—	—	—	—	—	—	—	.109	—
								(.070)	—
Child labor law by 1889?	—	—	—	—	—	—	—	−.153	—
								(.429)	—
Age limit	—	—	—	—	—	—	—	.051	—
								(.056)	—
Level of inspection	—	—	—	—	—	—	—	—.138	—
								(.174)	
State expenditures on public schools	—	—	—	—	—	—	—	.001**	.002**
								(.000)	(.000)
Exemptions (−)	—	—	—	—	—	—	—	−1.338*	−1.216**
								(.510)	(.448)
Model Statistics:									
AdjR2	.639	.032	.480	.459	.601	.556	.662	.589	.624
SE	1.08	1.74	1.28	1.30	1.10	1.18	1.03	1.14	1.09

Notes: Coefficients are unstandardized. Numbers in parentheses represent standard errors.

*$p < .05$.

**$p < .01$.

counter to Walker but at the same time in line with what many historians of U.S. social welfare might argue, our measures of possible cultural and ideological influences on mothers' pension enactments (per capita LITERACY and subscribers to the DELINEATOR magazine in the various states) proved to be jointly significant, as indicated in Model 1. During the early twentieth century, quite a few smaller, nonindustrialized and nonurban states, including the Rocky Mountain states, both were highly literate and saw mothers' pensions enacted early. Still, more remains to be learned about how culture and literacy mattered.

Arguments about how business, labor, and the composition of state work forces might have favored or impeded mothers' pensions also receive some support from our preliminary analysis, as revealed by the results for models 3–4. Well-entrenched state federations of labor were slightly associated with earlier enactments of mothers' pensions; at the same time, state-level enactments occurred much more slowly in those states where higher proportions of the gainful work force were female. Possibly, business interests in these states were especially resistant to mothers' pensions. Yet our variable PERCENT WORKFORCE FEMALE is very general; it may simply indicate an overall socioeconomic context in which many groups were either more reluctant (or less able) to promote mothers' pensions.

Turning now to the realms of politics and government, the results displayed in models 5–9 offer considerable comfort to proponents of various explanatory arguments. The best variables referring to features of electoral politics come to the fore in models 5–6, which suggest that states with more competitive elections, as well as states with stronger reformist third parties, were more likely to enact mothers' pensions relatively early. These results conform in certain ways to the arguments about mothers' pensions suggested by Gray (1973). Also worth underlining is the fact that FEMALE SUFFRAGE BEFORE 1920 did not prove significant when pitted against other electoral variables. Women's voting, in short, was *not* a primary reason for the early enactment of mothers' pensions in certain states during the 1910s.

Yet the importance of women's voluntary politics is suggested in model 7, which has a lower standard error and a higher adjusted R-squared than any other partial model in Table 1. Models 8–9 reveal support for certain hypotheses put forward by Drew (1987). These models show that higher state-level per capita expenditures on public schools are associated with earlier enactments of mothers' pensions; and they reveals that states without child-labor-law exemptions for widows and their children were slightly more likely to enact mothers' pensions sooner. In other words, states that had both the fiscal wherewithal and the statutory regulations to keep children in school and out of the wage-labor force were likely to be receptive sooner to groups arguing that widowed mothers should be given public monies to help them keep their minor children at home.

In sum, a first-cut analysis of our data offers considerable support for several explanatory perspectives. In order to tell more about "which theories work best" or how different hypotheses might be synthesized, we must move on to regression analyses that jointly test variables from different perspectives.

OVERALL MODELS: HOW MOTHERS' PENSIONS SPREAD ACROSS THE STATES

We used two different routes of statistical analysis to synthesize variables from the various explanatory perspectives we explored. In the first route, we tested all 24 independent variables listed in Table 1. We used correlation matrices to identify pairs or sets of highly interrelated and logically overlapping variables, setting aside seven that seemed redundant and not as clearly connected as were others to specific processes having to do with mothers' pensions.[8] Starting with 17 remaining variables, we ran successive regression models, dropping one variable at a time, until the three that remained were all significant. The result of this procedure was the same overall model (model 13) that we found using the second approach, whose steps and results are presented in Table 2. In this second approach, we tested together the ten variables that proved significant in the partial models discussed, moving through various steps (models 10–13) until all three remaining independent variables (WOMEN'S GROUP ENDORSEMENTS, STATE EXPENDITURES ON PUBLIC SCHOOLS, and PERCENT WORKFORCE FEMALE) were significant. Some might argue that model 12 is a better than model 13, because the standard error is a bit smaller. The substantive results in models 12–13 are reassuringly similar. We slightly prefer model 13 as our final model, because it is more parsimonious, and all of its variables are significant.

The substantive picture presented in model 13 is clear and convincing. Early endorsement of mothers' pension legislation by one or more women's groups in a state is very significantly associated with earlier enactment of mothers' pension legislation. Moving from no evidence of endorsement to the earliest endorsement(s) in 1912–13 advances the timing of enactment of mothers' pension legislation by about 4 years during the 1910s (and by about 10 years afterward). Another significant variable in our final model shows that states where females of 10 years and older constituted higher proportions of the gainful work force were relative laggards in adopting this sort of legislation. Moving from 15% of the work force female to 59% female pushes enactments backward in time about as much as early endorsements by women's clubs push enactments forward. Finally, higher per capita state expenditures on public schools (where the children of widows were supposed to go every weekday if their mothers could afford to care for them at home and keep them out of the wage-

TABLE 2

Overall Regression Models Predicting the Timing of Mothers' Pension Enactments

VARIABLE	MODEL 10	MODEL 11	MODEL 12	MODEL 13
Literacy	.013 (.039)	.007 (.031)	—	—
The Delineator	−.000 (.001)	—	—	—
Age of state federation	.006 (.016)	—	—	—
Percent work force female (−)	−.028 (.033)	−.026 (.025)	−.035 (.021)	−.046* (.021)
Party competition (−)	.560 (1.484)	—	—	—
Average reform vote	3.640 (4.251)	3.122 (4.003)	—	—
Density of women's clubs	.046 (.036)	.036 (.030)	.036 (.028)	—
Women's group endorsements	.400** (.121)	.470** (.102)	.497** (.088)	.542** (.086)
State expenditures on public schools	.001* (.000)	.001* (.000)	.001** (.000)	.001** (.000)
Exemptions (−)	−.586 (.394)	−.534 (.366)	−.475 (.351)	—
Model Statistics				
Adj. R^2	.778	.794	.800	.794
SE	.84	.81	.79	.80

Notes: Coefficients are unstandardized. Numbers in parentheses represent standard errors.
*$p < .05$.
**$p < .01$.

labor force) were also significantly associated with earlier enactments. Moving from the lowest to the highest levels of state expenditures advances enactments of mothers' pensions by about a half a year during the 1910s. The Appendix displays the values for each state of the three independent variables in our final model. These explanatory variables can be juxtaposed to the timing of mothers' pension enactments displayed in Figure 1 (and also given as exact years of enactment in the Appendix).

It is worth commenting on certain variables that were important in the partial regression models of Table 1, but dropped out of the final overall models reported in Table 2. One such variable was LITERACY. More literate states were more likely to enact mothers' pensions sooner. Not surprisingly, the more literate states were usually also the ones that spent generously on public schools. There were also more women's clubs per capita in the more literate states, and clubwomen in those states acted more quickly to promote mothers' pensions. We think that the relative literacy of adults in the various states probably did matter, even apart from public school funding; yet the effects of this variable may well have operated mainly through the existence and activities of local women's clubs.

Our most important electoral political variables, PARTY COMPETITION and AVERAGE REFORM VOTE, were likewise no longer statistically significant when pitted against the others in the final regression runs. We think this makes sense, too. Partisan electoral politics does not seem to have played a major role in the debates about mothers' pensions in most states. There certainly was a political process involved here, but it was not partisan electoral conflict. It was the rapid communication by the organized "woman movement" of a new ideal of public action to help mothers and children. Large majorities of state legislators enacted these laws in rapid waves across most of the states soon after the major, nation-spanning women's federations took up this cause, engaging in remarkably widespread and simultaneous public education and legislative lobbying about it.

In a few states, women's groups either never took up the cause or took it up slowly; and in some states, the efforts of women's organizations on behalf of mothers' pensions took a relatively long time to bear fruit. Our evidence suggests that the states where women's groups did not take up this cause, took it up late, or made slow headway were largely states where preexisting socioeconomic conditions were not conducive to pulling widows and their children out of the wage-labor force and where governments were fiscally less generous toward public schooling. Many (though not all) of the states that were slow to enact mothers' pensions were in the South. We checked in two ways to make sure that our final model 13 was not simply an invalid artifact of South-non-South differences among the states. When we reran our final model with a dummy variable for South-non-South, the operative variables remained significant, while the dummy variable was not significant. We also removed the 11 former Confederate states from our sample, and reran our final model for 37 northern states alone. The model remained significant (with a smaller standard error of .73). Two of the independent variables, WOMEN'S GROUP ENDORSEMENTS and STATE EXPENDITURES ON PUBLIC SCHOOLS, also remained significant. However, the third independent variable, PERCENT WORKFORCE FEMALE, did *not* prove significant when the final model was run for the northern states alone. This is not surprising, because many of the southern states were among the heaviest employers of female wage-laborers, which probably helps to account for why they were laggards in the enactment of mothers' pensions. In other words, we have found an operative variable that specifies how "southernness" operated in the politics of mothers' pensions.

Women's groups in the southern and nonsouthern states that were slow to enact mothers' pensions may well have been preoccupied with toughening child labor laws and improving the public schools. Mothers' pensions may not have seemed as important or realistic a policy priority for women's groups in such states as they seemed for women's groups in states that already had fewer women and children in the laborforce and strong child labor laws and public schools. In sum, our final model points both to the key political *actors*—the married women's federations—that worked for mothers' pensions and to the *contexts* of prior public policies and socioeconomic relations that influenced how hard or how successfully the women's groups pushed for these new laws.

REPRISE: INNOVATION IN STATE POLITICS

We have pointed to 14 predominantly nonurban and nonindustrial states that enacted mothers' pension laws much earlier than Walker's (1969, 1971) "composite innovation score" would have predicted. How well do the key variables in our final multiple regression models account for the unusual innovativeness of these 14 states? The reader who consults the Appendix will see indicated in small capitals the 14 states that enacted mothers' pensions much sooner than Walker would have expected.[10] It also gives the values for these states of all three of our final independent variables.

Virtually all of the states that enacted mothers' pensions unusually early in Walker's terms had relatively few females employed in the wage-labor force. At the same time, many of these smaller and less urban and industrial states were at or above the national mean in per capita expenditures on their public schools. Most telling is the information about women's politics. Early endorsements by women's associations are the best predictors we have in our overall model about mothers' pension enactments across the 48 states. Congruently, one or more women's groups were active between 1911 and 1915 in 11 out of these 14 unexpectedly innovative states.[11]

CONCLUSION

"The development of American social welfare policy was heavily influenced by women, who often saw themselves acting on behalf of women"—so argued Virginia Sapiro in a pathbreaking essay, "The Gender Basis of American Social Policy" (1986, 222). The empirical research we have reported here helps to support and refine Sapiro's assertion. Using innovative sources of quantitative data, we have demonstrated that the activities of women's voluntary groups were crucial in promoting mothers' pensions, early state-level statutes in the United States that authorized public welfare spending outside of poorhouses and other custodial institutions. Previous research on state-level legislative innovations has relied almost exclusively on conventional published statistics about industrialization, urbanization, and electoral and partisan factors. We have revealed that taking into account gendered variables (about women's voluntary politics and females in the laborforce) yields a very different—and more convincing—explanation of the pattern of state enactments of mothers' pensions between 1910 and 1935.

Our study also pinpoints exactly *which kinds* of female voluntary associations mattered most in the origins of mothers' pensions. Historians have often described the political activities of highly educated professionals, many of them single women, who were located in midwestern and northeastern urban social settlements and in staff-run, elite advocacy organizations like the National Consumers' League. (For a recent theoretical extrapolation based on the traditional historical wisdom, see Gordon 1992.) Yet our research on mothers' pensions suggests that the women's groups that mattered most in this case were not the professional advocacy groups but, rather, geographically widespread federations of local clubs peopled mostly by married women. The General Federation of Women's Clubs and the National Congress of Mothers were the widespread federations that figured most centrally in our data on voluntary groups. It remains for further research to show whether these same widespread women's federations were equally influential in promoting other legislative reforms of the early twentieth century (such as protective labor legislation for women workers). But we have demonstrated their centrality to the enactment of America's first explicit social welfare program for mothers and children.

The centrality of the General Federation and the National Congress in our data solves one of the key puzzles posed at the start of this article: Why did mothers' pensions spread so quickly across many of the states during the early 1910s, certainly much more quickly than either Walker's (1969, 1971) or Gray's (1973) models of legislative innovation would have led us to expect? As far-flung federations with local organizational roots in both more and less industrial (and more and less urban) parts of the United States, the General Federation and the National Congress could rapidly promote a nearly nationwide debate about the moral desirability of providing public aid to worthy widowed mothers and their children. In the parlance of today's political science, they were powerful "agenda setters" in the social politics of the early twentieth-century United States (Kingdon 1984). Even without the right to vote, American women working through these federations could spread new ideas and legislative proposals quickly. Thus, women's voluntary federations could overcome the inertia otherwise built into a U.S. political system that prior to the New Deal, left most decisions about social legislation to male officials in 48 state capitals.

Future students of social welfare politics need to realize that political parties, elections, and bureaucratic and legislative maneuverings are not the totality of such politics. Movements and groups that can achieve widespread social communication and can use moral arguments to set agendas of public and legislative debate may matter as much as, or more than, voting and elections. This may be especially true in a polity such as the United States, where bureaucracies and programmatic political parties are comparatively weak and where multiple legislatures may have to be inspired to act.

APPENDIX

Table A-1 (see page 714) gives the values for all states of our three final independent variables and highlights in block capitals the 14 states that enacted mothers' pensions much sooner than would be predicted from Walker 1969 and 1971.

TABLE 1-A

Dependent and Independent Variables for 48 States

YEAR OF ENACTMENT OF MOTHERS' PENSIONS	ENDORSEMENT BY WOMEN'S GROUP(S) IN STATE?	PER CAPITA EXPENDITURES ON PUBLIC SCHOOLS[a]	PERCENT WORK FORCE FEMALE[b]
1911			
Illinois	[1914–51]	$21.05	22.7
1913			
California	1912–13	24.01	22.3
Colorado	1912–13	22.99	21.1
Idaho	1912–13	21.89	15.0
Iowa	1912–13	19.14	17.9
Massachusetts	1912–13	20.59	35.2
Michigan	1912–13	18.67	19.7
Minnesota	1912–13	20.54	22.8
Nebraska	no evidence	21.49	16.9
New Hampshire	1912–13	13.19	30.4
New Jersey	[1914–15]	22.03	27.1
Nevada	1912–13	21.78	19.0
Ohio	1912–13	19.04	20.8
Oregon	1912–13	19.30	20.1
Pennsylvania	1912–13	17.95	23.8
South Dakota	1912–13	22.30	17.2
Utah	1912–13	26.45	16.7
Washington	1912–13	26.71	19.7
Wisconsin	1912–13	16.34	21.6
1914			
Arizona	no evidence	15.56	20.3
1915			
Kansas	1914–15	17.61	15.0
Montana	1914–15	19.66	19.3
New York	1912–13	21.00	29.9
North Dakota	no evidence	27.73	18.7
Oklahoma	1914–15	7.11	17.0
Tennessee	1912–13	5.56	25.3
West Virginia	1914–15	12.54	15.1
Wyoming	1914–15	14.73	17.1
1916			
Maryland	1916–17	10.33	28.8
1917			
Arkansas	no evidence	7.06	35.4
Delaware	1914–15	9.55	25.8
Maine	1916–17	13.35	23.8
Missouri	1914–15	14.26	19.3
Texas	1914–15	9.43	28.8
Vermont	1916–17	15.18	22.4
1918			
Virginia	no evidence	7.61	26.1
1919			
Connecticut	1918–19	16.09	30.2
Florida	1916–17	8.13	32.3
Indiana	1918–19	17.63	16.9
1920			
Louisiana	no evidence	7.78	35.3
1923			
North Carolina	no evidence	4.85	41.0
Rhode Island	no evidence	16.94	36.0
1928			
Kentucky	no evidence	7.56	20.4
Mississippi	1918–19	5.32	57.1
1931			
Alabama	no evidence	4.17	48.8
New Mexico	no evidence	5.90	16.3
Without laws in 1935			
Georgia	no evidence	5.48	44.7
South Carolina	no evidence	4.49	59.4

Note: Brackets indicate that the known endorsements by women's groups came after initial mothers' pension legislation was passed. For further information on the women's group endorsements, see Skocpol 1992, table 9 and app. 2.

[a] Range = $4.17–$27.73, mean = $15.17

[b] Range = 15%–59.4%, mean = 25.8%.

Notes

We were generously helped by others in the process of producing this article. John Sutton (1988) shared data sets from his earlier research. Many people commented on earlier drafts, helping us to improve our analysis and arguments. For such assistance we are grateful to the members of the Workshop on American Political Development of the Center for American Political Studies at Harvard University and to Edwin Amenta, Morris Fiorina, Virginia Gray, Gary King, Eileen McDonagh, Ann Shola Orloff, William Parish, Kay Schlozman, Richard Sobel, Rick Valelly, and Sidney Verba.

1. To make certain that our final results were not distorted by the use of the ordinal dependent variable, we retested model 13 reported in Table 2 below, substituting a continuous dependent variable (the number of years before 1935 that each state enacted mothers' pensions). Reassuringly, the final model remained statistically significant, with all three independent variables significant and behaving the same way. The model using the continuous dependent variable did, however, have a standard error of 3.48, more than four times larger than the standard error of model 13.

2. Since the early studies, political scientists have refined their claims, arguing, for instance, that socioeconomic variables may be more important for distributive policies like education and less important for redistributive measures like Aid to Families with Dependent Children (Cnudde and McCrone 1969; Fry and Winters 1973; Sharkansky and Hofferbert 1969). More recently, scholars have argued that economic and political explanations necessarily complement one another. While one can find evidence of this synthesis in Lockard (1959), it did not take hold in the literature until the late 1970s (Jennings 1979; Peterson and Rom 1989; Plotnick and Winters 1985; Tomkins 1975). Although few scholars now make sweeping claims about the ability of industrialization to explain state-level variations, economic variables continue to play a major role in their models.

3. The 1910 U.S. Census did not total agricultural production by state, because of the difficulty of estimating the value of crops that farmers fed to their livestock. Thus, an approach to calculating state wealth that adds together the values of industrial and mining production with the values of individual agricultural products (e.g., milk, cotton, livestock, grains, orchard fruit) inevitably double-counts some agricultural production.

4. We computed an average PARTY COMPETITION score for each state based on the extent of competition in 1910, 1912, 1914, and 1916. For each year, the score represents an average of three numbers: the percentage of seats in the state house of representatives controlled by the major party, the percentage of seats in the state senate controlled by the major party, and the percentage of the gubernatorial vote won by the governor's party. The higher the average of these ratios, the less party competition. A score of 1.00 indicates complete dominance of electoral politics by one party. Thus, a negative slope for the regression coefficient indicates a positive correlation between party competition and the timing of enactment. Our methodology assumes that a two-party system in which one party wins 60% of the seats/votes and the other party wins 40% is as competitive as a three-party state in which one party wins 60% and the other two parties win 20% each.

5. Gray argues specifically that Progressive party strength was a good predictor of early mothers' pension enactments (1973, 1183). We tested variables referring to Progressive and/or Socialist party strength in 1912 and found that they were much weaker predictors than our overall measure of "average reform vote."

6. We also tested variables referring to each electoral reform individually.

7. The reader may wonder whether women's clubs sometimes *claimed* to have endorsed mothers' pension laws after they were enacted—in effect, "jumping on the bandwagon" after the fact. Available sources of data do not allow us to rule out this possibility in every single case, but we can say that women's groups usually claimed to have endorsed mothers' pensions and started lobbying for them well *before* state legislatures passed these laws.

8. The variables set aside on account of high intercorrelations with other variables were: LITERACY; URBANIZATION; PERCENT CHILD LABORERS; AVERAGE DEMOCRATIC PARTY STRENGTH; ELECTORAL REFORMS; STATE GOVERNMENT REVENUES; and AGE LIMIT for child labor in manufacturing.

9. Innovation scores were computed for states enacting mothers' pension laws following Walker's (1971) normal procedure. Then, differences from his composite scores for each state were calculated. The 14 states listed in the Introduction were the only ones that were .4 or more points different from Walker's average scores (which range from 0 to 1). The states were listed in order (starting with Nevada), according to how much earlier they enacted mothers' pensions than Walker would have expected. There were no states that were extreme laggards in Walker's terms—not surprising, given that mothers' pensions spread across the states much more quickly than most of the laws Walker studied.

10. The three unusually innovative states where women's groups did *not* endorse mothers' pensions prior to their enactment are worth brief comment. Arizona enacted its 1914 mothers' pension law by popular referendum, and we have found no evidence of endorsements by women's groups. In Illinois, women's groups definitely endorsed mothers' pensions at least by the mid-1910s and then worked steadily for improvements in these laws. But the original 1911 law—the first in the nation, along with a county-level measure in Missouri—was enacted by the state legislature at the urging of a Chicago juvenile court judge. The third odd state is Nebraska. We have searched available records as thoroughly as we can from a distance but have found no evidence of women's group endorsements of mothers' pensions. Yet local women's clubs were unusually dense in this state. It may be that some were active on behalf of the mothers' pension law that passed in 1913, but we have not found the evidence.

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ESTABLISHING THE MICRO FOUNDATIONS OF A MACRO THEORY: INFORMATION, MOVERS, AND THE COMPETITIVE LOCAL MARKET FOR PUBLIC GOODS

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The Tiebout model of competition in the local market for public goods is an important and controversial theory. The current debate revolves around the apparent disparity between macro empirical studies that show greater efficiency in the supply of public goods in polycentric regions compared to consolidated ones and micro evidence of widespread citizen–consumer ignorance, which has been used to argue that individual actions cannot plausibly lead to efficiency-enhancing competition between local governments. We argue that competitive markets can be driven by a subset of informed consumers who shop around between alternate suppliers and produce pressure for competitive outcomes from which all consumers benefit. Using data from a survey of over five hundred households, we analyze the role of these marginal citizen–consumers and incorporate the costs of information gathering and the strategic interests of local governments into the competitive market model.

In 1956, Tiebout laid the foundation for a competitive model of local government. Tiebout argued that by shopping around across communities, mobile citizen–consumers increase the efficiency and the responsiveness of local governments (also see Ostrom, Tiebout, and Warren 1961). In this market-oriented approach to local public goods, "mobility provides the local public goods counterpart to the private market's shopping trip" as citizens choose to locate in a community whose taxes and services most closely match their own individual tastes (Tiebout 1956, 422). Although many differences between a pure market for private goods and a local market for public goods limit the effects of competition,[1] a substantial body of research based on the Tiebout model has shown that allocative efficiency is increased through the spatial sorting process and that local governments in polycentric regions deliver services more efficiently than do monocentric local governments (DiLorenzo 1983; Parks and Ostrom 1981; Schneider 1986, 1989; Sjoquist 1982; Zax 1989).

While these aggregate-level findings are compelling, scholars have attacked the Tiebout model from a variety of perspectives. At its very core, Tiebout's model of "pure" competition is based on a set of highly abstract principles (unrestricted mobility across an urban area situated on a featureless plane, perfect information, etc.). In the almost four decades since its publication, numerous researchers have criticized the extreme conditions detailed in these principles. However, it is not uncommon for the robustness of abstract models like Tiebout's to be tested through the progressive relaxation of the highly restrictive assumptions built into the original model. In this manner, models are made more complex, more nuanced, and more congruent with empirical realities. (For a discussion of this process, see Bendor and Hammond 1992, 302.)

Oakerson and Parks (1988), for example, criticize the Tiebout model's exclusive reliance on mobility. They argue that political activity, or "voice," is at least as important as mobility (exit) in determining local government service–tax decisions. Others argue that the role of local governments in the Tiebout model is too passive. In the Tiebout model, government policies are "more or less set," but governments actually engage in strategic behavior in determining their services and taxes. Peterson (1981), Schneider (1989), and Stein (1990), among others, introduce a more active government dimension to the Tiebout model.[2]

While these debates continue, Tiebout's notion that citizen–consumers "vote with their feet"—the central theoretical construct in the model—has recently attracted the most attention. Critics have raised questions about several dimensions of this individual-level aspect of the model. First, the Tiebout model assumes that citizens are perfectly mobile—that they can easily exit a community that does not satisfy their preferences and enter a new community that offers a service–tax package that they prefer. In contrast to the frictionless plane of the Tiebout model, numerous real-world constraints on the mobility of individuals limit exit as Orbell and Uno (1972) argued long ago. But it is what Lowery and Lyons (1989) call the "information assumption" of the model that has recently attracted the most attention (see Lowery, Lyons, and DeHoog 1990; Lyons and Lowery 1989; Sharp 1984). Lowery and Lyons argue that the Tiebout exit–entry process requires high levels of information on the part of individual citizen–consumers—that for the Tiebout model to work, citizen–consumers must have accurate knowledge of the alternative local public service–tax packages being offered elsewhere in their region.

Using a carefully matched sample of residents in a polycentric and a monocentric metropolitan region, Lowery and Lyons investigate several of the individual-level information assumptions that they argue underlie the Tiebout model. Specifically, they question whether individuals are aware of alternative service–tax packages in other local governments, whether they are motivated to exit a jurisdiction if its service–tax package does not align with their preferences, and whether they make their relocation decisions based on the alternative service–tax packages offered by municipalities.

Lowery and Lyons find little empirical evidence that any of these assumptions hold.[3] They conclude that the organization of local government and the range of services offered in a metropolitan region "just do not seem to influence attitudes and behaviors in the gross manner suggested by the Tiebout model, [and] many of the aggregate-level findings on service delivery may have to be rethought" (1989, 94–95).

The Macro–Micro Disparity

Lowery and Lyons pose a fundamental theoretical question: Is it possible to reconcile macro evidence that illustrates the effects of Tiebout-like market mechanisms on local government efficiency with evidence that at the micro level, key assumptions are violated? Although macro–micro disparities are not uncommon in social science, this matter is probably most familiar to those who have followed recent debates in macroeconomics between Keynesians and the micro–level advocates of rational expectations models. On the one hand, we could argue that as long as macro models predict well, nothing is gained by comparing their underlying assumptions to micro evidence. This position is illustrated in the famous "as if" dictum credited to Milton Friedman (1953). On the other hand, others argue that since science is concerned with accurate explanation, the "as if" dictum is simply not acceptable.

Blinder, in reviewing the conflict in economics notes that "neither side is hostile either to first principles or to factual accuracy. We all agree that the ideal macro theory would be built up logically from first principles and would explain the data well. But we also agree that such a theory is a long way off" (1989, 123–24). Blinder goes on to note that good science need not always be built on solid micro foundations. Thermodynamics, chemistry, and medicine have all accomplished much as sciences without strong micro theory. On the strength of this conclusion, Blinder argues that empirical regularities formulated and tested directly at only the macro level clearly contribute to the development of the sciences.

Despite Blinder's argument, social scientists interested in the underlying social and political processes that give rise to observed data are no doubt frustrated with theory that cannot reconcile macro and micro evidence. In this research, we try to place the aggregate-level data that support the competitive Tiebout model on a micro foundation that is built on a better understanding of markets and the role of consumers than is evident in most political science. We believe that the current debate over the micro assumptions of the Tiebout model is built on a faulty understanding of how markets work. In particular, research on the "information assumption" has not focused on how markets can be driven by only a subset of actors and by the information search processes in which they engage.

We believe that a fundamental source of problems in correctly specifying the micro foundation of the Tiebout model is the belief that *all movers* must hold information about comparative service–tax packages. Indeed, Lowery and Lyons go even further and imply that *all citizens* must have accurate information about local goods and services. In fact, studies of private-sector markets show that only a subset of consumers, especially those with high incomes, are likely to gather information about their purchases (Claxton, Fry, and Portis 1974; Katona and Mueller 1955; Newman and Staelin 1972; Thorelli and Engledow 1980). More importantly, these studies show that this small percentage of buyers can effectively drive a market toward a competitive outcome.[4] Paralleling these empirical findings for competitive private-sector markets, we argue that a market for local public goods can be driven toward a competitive outcome by only a subset of informed citizen–consumers. In other words, for the local market for public goods to be competitive, only a subset of movers need to have information on the various packages of goods and services that local governments offer. This is particularly true if these knowledgeable movers possess high incomes.

The Role of Consumers. There is a critical difference between the role of the average consumer (whose level of information may be low) and the informed, or *marginal,* consumer. The failure to understand the importance of marginal consumers and the costs that different classes of citizen–consumers face has fundamentally flawed research into the information that individuals need to drive a Tiebout-like market. Much of the recent research on movers and the Tiebout model is overly focused on the threat of *exit.*[5] We believe that the *entry* decisions, not the exit decisions, of movers are more theoretically attuned to Tiebout's description of mobile citizen–consumers. Most residential moves are motivated by factors that have nothing to do with public services; rather, they are motivated by changes in employment or changes in life circumstances. But once forced to shop around for a new house, a new community, and an attendant new local government, the *marginal* costs of gathering information about local public services are low. Thus, actual movers, as they choose among their entry options, may be as close to Tiebout's "frictionless mover" as we will find in the real world. But no search process can reveal all aspects of the package of taxes and services that municipalities offer in the local market for public goods. In turn, we discuss the "search" and "experience" aspects of local public goods; and these characteristics affect the micro and macro outcomes in the local market.

Pressure Toward Competitive Equilibria. Our empirical focus is at the individual micro level, establishing the comparative levels of knowledge that recent movers and more established residents actually have about local government service–tax packages. But our theoretical focus is on the link between these results and other market processes. We seek to identify why municipalities respond to consumer search and how the behavior of citizen–consumers can produce macro forces that push the local market for public goods toward competitive outcomes.

We shall discuss these issues in detail, concentrating on the difference between marginal and average consumers. We then develop the outlines of a more rigorous theoretical approach to the study of the individual-level assumptions underlying the competitive model of the local market for public goods and discuss how the particular nature of this market affects competitive outcomes. In the development of our argument, we update the Tiebout model to respond to many of the criticisms that have subsequently been leveled at it, incorporating the costs of information gathering, the costs of mobility, and the strategic interests of municipal governments. With this theoretical structure in place, we use data from a survey of over five hundred recent movers and established residents in Suffolk County, New York, to present evidence on how a class of informed citizen–consumers may satisfy some of the individual-level assumptions that *actually* underlie the role of citizen–consumers in the local market for public goods.

THE ROLE OF THE MARGINAL CONSUMER

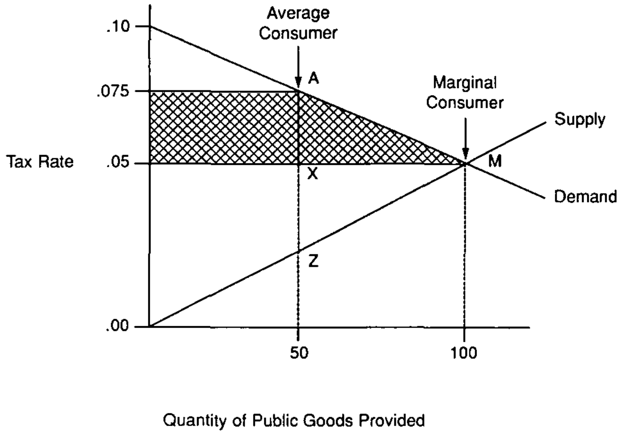
By analyzing the responses of a sample of *all* individuals in two metropolitan areas, Lowery and Lyons have in fact based their criticism of the Tiebout model on the behavior of the "average consumer." Since the average citizen does not know much about local services and taxes, Lowery and Lyons conclude that a competitive Tiebout-like model of the local market for public goods cannot be operative. But the focus on the average consumer as the driving force in markets is misplaced: economists recognize that both theory and empirical research should focus on the marginal consumer. Rhoads argues that in many markets the marginal consumers are the most careful and best-informed shoppers, whose actions generate "competitive pressures that help keep prices reasonable for less-informed, non-searching consumers as well" (1985, 144; see also Thorelli and Engledow 1980 on "information seekers" and Feick and Price 1987 on "market mavens"). This set of informed consumers acting in their own self-interest help create an efficient market. Schwartz and Wilde (1979) specify the argument further. For them, "the presence of at least some consumer search in a market creates the possibility of a 'pecuniary externality': persons who search sometimes protect nonsearchers from overreaching firms" (p. 638). More to the point, they argue that "*the conventional analysis asks the wrong question*. Rather than asking whether an idealized individual is sufficiently informed to maximize his own utility, the appropriate normative inquiry is whether competition among firms for *particular groups of searchers* is, in any given market, sufficient to generate optimal prices and terms for *all consumers*" (ibid., emphasis added). In applying this argument to the Tiebout market, replace the term *firms* with *municipalities*.[6]

To illustrate this point in the context of a private good, consider the decision to buy a car. Most cars last several years. For much of the time between car purchases, a consumer need not know anything about the costs or benefits of different models of new cars: the consumer is not looking to buy a car and there is no benefit in expending resources to gather information. However, when consumers do enter the car market, they engage in extensive search about the cost and quality of various car models. A survey of *all* consumers would show that despite the millions of dollars spent by car companies advertising their products, the *average* consumer has little accurate information about different car models. Yet, the car market is highly competitive, because it is driven by the marginal consumer, the relatively small proportion of the population that is "in the market" at any given time and for whom the benefits of search outweigh the costs of gathering information.

This argument can be made graphically. Figure 1 shows a hypothetical supply-and-demand curve, where the "commodity" being traded is local service–tax packages. We assume that price discrimination is not feasible in the depicted market, as is usually the case by law for the services and taxes that cities offer to their residents. The market-clearing tax price for the package of public goods is $.05 per $1000 of home value, and one hundred units will be purchased. But the average consumer in this market is actually willing to pay a tax rate of $.075 for the public goods.[7] In contrast, the marginal consumer will not purchase the good at this tax price (perhaps because this consumer is better informed about the real worth of public goods and the taxes being charged by other municipalities). In a fully competitive market, the marginal consumer will drive the tax price down to $.05 per unit, which is what *every* consumer, including the average consumer, will pay. Of critical importance, note that while the marginal consumers who engage in extensive search may be a small, homogeneous, high-income group, their actions impart benefits to all consumers, many of whom will be of low or middle income. We recognize that the local market for public goods is not fully competitive, so that there is variation in the tax prices that local governments

FIGURE 1

The Effects of the Marginal Consumer on Efficiency in the Local Market for Public Goods



Note: While the "average consumer" is willing to pay a tax rate of $.075, the well-informed "marginal consumer" drives the market tax price to $.05. The triangle, MAX, represents the surplus that citizen–consumers gain from the pressure toward a competitive (efficient) equilibrium. The triangle MZX also represents an efficiency gain, but one that accrues more directly to producers. The degree to which this producer surplus is transferred back to citizen–consumers is a function of the exactive capacities of local bureaucrats (see Niskanen 1971; Schneider 1989). The rectangle A–X–.05–.075 represents an additional transfer from municipalities to citizen–consumers. The pecuniary advantage generated by the marginal citizen–consumer is the sum of MAX plus this rectangle plus whatever proportion of the triangle MXZ is returned to consumers. If there is no price discrimination, the gain is divided among all citizen–consumers.

charge and the services that they provide. But in either the public or the private market, the pressure toward more competitive pricing is driven by the same mechanism: by having more accurate information about prices, the marginal consumer drives the market toward a competitive level.

The Search for Information

The low level of information held by the "average consumer" is the strongest criticism leveled against the Tiebout model. How, critics ask, can a Tiebout market work when the vast majority of citizens do not possess information about the services and taxes that local governments offer? As with other kinds of political activity, many citizens may be "rationally ignorant" (Downs 1957). But given the role of marginal consumers, we do not need to demonstrate that large numbers of people have high levels of information; we need only identify specific classes of individuals who know more about services and taxes.

Movers as Marginal Consumers. One class of individuals who may help drive the Tiebout market are actual movers. Most citizens do not exit their community because of dissatisfaction with the quality of the local service–tax package. Rather, moves are overwhelmingly driven by employment considerations or changes in family status. In short, the decision to move is largely exogenous to the local market for public goods (Fields 1979; Long 1974. But on the impact of welfare payments on the mobility choices of low-income families, see Blank 1985; Gramlich and Laren 1984; Peterson and Rom 1989.) However, once an individual, driven by personal or employment reasons, is facing the high transaction costs of moving, the incremental costs of gathering information on local services and taxes are low.

How likely is it that movers will engage in search for public service–tax packages that they prefer? Consumers utilize cost–benefit calculations in their search processes (e.g., Lanzetta and Kanareff 1962; Stigler 1961); and as the value of a good increases, so does the number of individuals who engage in comparative shopping (Beatty and Smith 1987; Bucklin 1966, 1969; Newman and Staelin 1972; Punj and Staelin 1983; Udell 1966). Houses are the most expensive purchases that most individuals ever make, and they have correspondingly strong incentives to gather information.

The marginal cost of gathering information is reduced by the role of intermediaries who provide information to consumers at a low cost (Friedman 1962; Rhoads 1985). For example, the costs that movers face for gathering information about alternative communities may be reduced by real estate agents, who provide information about schools, local services, and taxes in addition to information about specific houses. Citizens also utilize information gathered by others. According to Kozol, the New York State publication *Statistical Profiles of Public School Districts* became "a small best-seller" (1991, 120).

Moreover, at the time of major real estate transactions, movers are also most sensitive to the future capitalization of housing value and will resist purchasing housing in a community that does not offer a competitive stream of capitalization because of poor services and high taxes. Thus, entry decisions are more likely to be driven by local services and taxes; and in turn, it is at the point of *entry,* not *exit,* that the Tiebout mechanisms are most likely to come into play.

Search and Experience. But even if movers have incentives to search, they face limits in their ability to gather accurate information. Economists distinguish between search goods, whose the quality can largely be determined prior to purchase, and experience goods, which must be consumed in order to learn their quality (Barzel 1982; Nelson 1970; Weimer and Vining 1992). Markets for experience goods can be inefficient, since consumers either cannot verify product quality at the time of purchase or must expend relatively large resources (compared to the price of the good) to do so.

Wilde shows that even relatively simple private goods possess some "search characteristics" and some "experience characteristics" (1981, 1123). For the complicated package of local public goods that Tiebout's shoppers purchase along with their residential location, we expect to find some characteristics best ascertained through search and others that require experience. For example, the local tax rate, the student–faculty ratio in local schools, and the number of parks in a community may be easily assessed at low search costs. Far greater search costs are required to determine the stability of the local budget and the likelihood of future tax increases, the ability of local schools to place students in quality colleges, and the safety of the parks at night. These aspects of local public goods may be best learned through experience. Given relatively lower search costs for movers, we expect movers to gather much of their information about public services and taxes through search. Given the role of experience in assessing elements of the local service–tax package, we expect that established residents will increase their accurate knowledge of local public goods over time.

While we believe that these are the most important dimensions of search and experience to properly specify the Tiebout market, in Appendix A, we provide a fuller summary of relevant arguments from economic and business studies of information and search in markets for private goods.

THE EFFECTS OF SEARCH ON COMPETITIVE OUTCOMES

Consumers face different configurations of producers in private markets and in the local market for public goods. These configurations influence what incentives individuals have to engage in search and what effects search will have on the competitiveness and responsiveness of producers.

In a market characterized by a single provider, consumers have no incentives to engage in search, because there are no choices among goods. And the provider has little or no incentive to be efficient or responsive in its behavior. As is well known, the outcome is a monopoly price higher than the competitive equilibrium. Consider next a market that is characterized by multiple producers but one in which consumers are all ignorant of the quality of goods or the prices that alternate producers charge: that is, consumers are engaging in either no search or low-quality search. As in the monopoly market, producers have few incentives to charge anything less than the monopoly price: the presence of multiple producers *alone* does not produce efficiency gains over the monopoly market. However, more efficient outcomes begin to emerge as soon as some consumers engage in search and gather information about the quality and prices of goods provided by alternate producers. Wilde and Schwartz note that "the likelihood of competitive equilibria obtaining varies directly with the number of consumers who visit more than one firm [read *local government*] and with the number of firms such 'comparison shoppers' visit" (1979, 551). And even a little comparative information can go a long way. As Bucklin notes, "the value to the buyer of even a little information is striking" (1969, 416).

The Incentives of Municipal Governments in the Local Market for Public Goods

To earn profits, business firms respond to informed consumers of private goods. We argue that there is similar pressure for local governments to respond to informed consumers of local public goods. But there are distinctive characteristics of the local market for public goods that affect how these market mechanisms come into play. While models of markets for private goods alert us to important underlying mechanisms of competition and efficiency, they must be modified in order to help us understand processes in the provision of public goods, especially local public goods.

Tiebout's original formulation relegated municipal governments to a secondary role, portraying them as inactive agents. This simplifying assumption may have resulted from Tiebout's concern for a "pure" theory of competition in metropolitan regions. In the case of pure competition for private goods, the strategies and actions of suppliers are irrelevant to understanding market outcomes. By analogy, in a purely competitive public market, the actions of government need not be modeled. But for many reasons, especially since the local market for public goods is not purely competitive, municipal governments are not passive actors in the local market for public goods (see Ostrom, Tiebout, and Warren 1961). From the perspective of local public finance, not all citizen–consumers are equally valuable to communities, and municipalities have incentives to fashion service–tax packages to appeal to certain individuals, as against others. In fact, the "average consumer" may be of no particular interest to competitive cities. Rather, current community residents have incentives to use the tools of their local government to maximize the level of local wealth in their community. Present members of a community are interested in making their community more attractive to an individual with above-average wealth. Given a fixed tax rate and legal constraints on limiting access to local public services, individuals buying above-average-cost housing in a community pay what Buchanan has called a "net fiscal dividend" to present residents (Buchanan 1981). According to Peterson (1981) and Schneider (1989), one of the most effective ways in which to attract such residents is by offering an efficient mix of services and taxes for those services important to above-average income individuals.

In turn, even if only these above-average-income individuals make their entry decisions based on service–tax packages, a market like that described in the Tiebout model could still operate as predicted. That is, given the structure of the local market for public goods, if only relatively wealthy individuals shop around, their actions may be sufficient to produce competitive service-and-tax decisions by local government and the process illustrated in Figure 1 can be extended to the local market for public goods. Obviously, we cannot gather all the information fully to analyze equilibria in different metropolitan market settings. However, we take an important first step by focusing on the search processes of the citizen–consumer in the local market for public goods. Specifically, we use empirical survey data to understand how movers and nonmovers gather comparative information about local government services in a polycentric market setting.

ANALYSIS AND RESULTS

To address some of the issues raised by our theoretical perspective, we conducted two telephone surveys of households in Suffolk County, New York. This county, with a population of approximately 1.3 million, has a polycentric structure of government, consisting of 10 towns, 24 incorporated villages, 71 school districts, and innumerable special districts.

The first survey was designed to test the knowledge that established (nonmoving) householders had of local government services. The second survey tested the knowledge of householders who had recently bought houses in Suffolk County. Appendix B describes in detail the methods used and the demographic profile of these surveys.

We shall focus on school district expenditures and taxes.[8] We consider respondents to be "informed" if they accurately ranked their own district on both expenditures and tax rates relative to other school districts in Suffolk County.[9] We first compare the accuracy of the information held by movers categorized by their level of wealth and interest in local schools. We then present a probit analysis of the influence of these characteristics of movers on accuracy. In a second probit analysis, we pool recent movers and established residents to examine the search and experience aspects of accurate information about local services. Then, we discuss the quality and sources of information search used by movers. Finally, we relate accurate knowledge to the use of political voice within a community.

Not surprisingly, our data show that the level of accurate information held by all citizens is low. Overall, 21% of our respondents accurately evaluated the ranking of their schools. This is about 50% higher than what would be expected by chance. Movers taken as a whole actually had less accurate information about their schools than did nonmovers: 19% of movers accurately judged the expenditures and taxes of their school district, compared to 24% of established residents.[10] But the accuracy of information increased with theoretically important characteristics of movers.

Local governments have strategic interests in attending to the interests of wealthy individuals. Thus if these individuals seek out and hold more accurate information about services and taxes, then pressure for efficiency may result, regardless of the "average" level of information held by all citizen–consumers or held by all movers. To examine differences in information, we divided our sample of movers and nonmovers into two categories: those with household incomes above the average for the sample ($50,000) and those with incomes below the average.[11] Compared to low-wealth respondents, high-income respondents were significantly more likely to have accurate information about their school districts: while low-income movers proved accurate only 12% of the time, high-income movers were accurate 28% of the time.[12]

TABLE 1

Factors Influencing the Accuracy of Information about Local School Expenditures and Tax Rates: Probit Analysis of 224 Recent Homebuyers

INDEPENDENT VARIABLE	COEFF.	S.E.	T	P > T
School important	.426	.244	1.75	.08
Kids in school	.408	.222	1.84	.07
Income	.763	.225	3.40	.00
Constant	−1.818	.270	−6.74	.00

Note: N = 224; chi-squared = 20.4, $p < .001$. The dependent variable equals 1 when respondents correctly identified both their school tax rate and expenditures as average, above average, or below average (42 of the cases)—0 otherwise. The *school important* variable equals 1 if respondents said that local schools were very important to them. The *kids in school* variable equals 1 if respondents have children of school age, 0 otherwise. The *income* variable equals 1 if the household income exceeds the sample median, 0 otherwise.

Marketing studies shows that certain consumers are more "involved" with some purchases and that these individuals make markets (see Appendix A). We assess the relative importance of local government services to the location decisions of movers by means of two questions. First, we asked movers to name the factors that were important to them in choosing the community in which they chose to buy their house. Forty percent of movers volunteered that the quality of schools, parks, and other local services affected their choice of community. Second, we asked movers directly whether the quality of local schools was important to their choice of community. The percentage of respondents who said *yes* is high: 68% of movers said that schools were very important. Clearly, movers do attend to information about local services, especially schools, when making their entry decisions.

In Table 1, we present a probit analysis testing the influence of attentiveness to schools, the presence of school-age children, and income level on the accuracy of information held by movers. The results confirm that even with other factors included, higher-income movers are much more likely to hold accurate information about their local school district. Movers are also more accurate when they report caring about local schools (at $p < .08$) and when they have children in the schools (at $p < .07$).

Probit allows for a sensitivity analysis to determine how changing the values of independent variables affects the probability that an individual held accurate information. For all movers in Table 1, the probability of having accurate information is 19%. But if all

TABLE 2

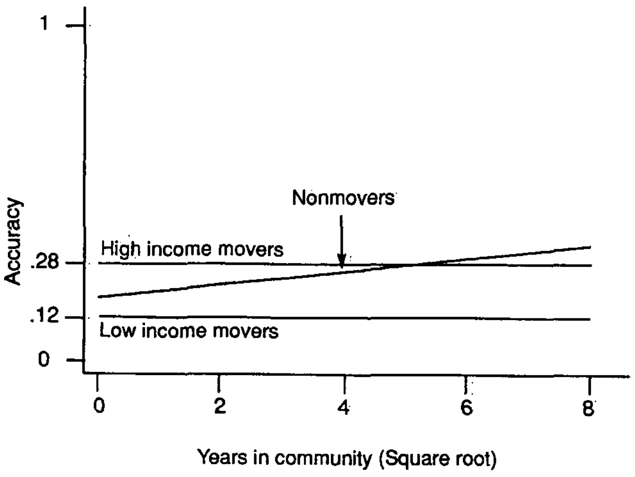
Factors Influencing the Accuracy of Information about Local School Expenditures and Tax Rates: Probit Analysis of 422 Homeowners

INDEPENDENT VARIABLE	COEFF.	S.E.	T	P > T
Time in community	.078	.031	2.55	.01
Kids in school	.295	.144	2.05	.04
Income	.370	.140	2.64	.01
Constant	−1.162	.135	−8.63	.00

Note: N = 422; chi-squared = 16.6, $p < .001$. The dependent variable equals 1 when respondents correctly identified both their school tax rate and expenditures as average, above average, or below average (89 of the cases)—0 otherwise. The *time in community* variable is the square root of the number of years of residence in the present community (0 for recent movers). The *kids in school* variable equals 1 if the family has children of school age. The *income* variable equals 1 if the family income exceeds the sample median, 0 otherwise.

FIGURE 2

Search, Experience, and Accuracy



movers had high income, accuracy would increase to 26%. If all movers cared very much about local schools, accuracy would increase to 22%; and if all movers had school-age children, accuracy would increase to 27%. But if all movers had high income, cared about schools, *and* had school children, the accuracy rate would jump to 41%—triple the chance accuracy rate. We take this as evidence of a class of mobile and valuable residents who know about the relative performance of their schools.

We present a second probit analysis in Table 2 to compare the effects of income and school-age children on accuracy for the whole sample of Long Island residents, including nonmovers. Because local public goods may have a significant experience dimension, we test the effects of length of residence in a community on the probability that a respondent has accurate information. As Table 2 reports, accuracy does increase with time in a community, reflecting the experience aspects of local public goods. (Bledsoe and Stoker [1992] find a similar result for other kinds of information about local politics.) For the entire sample (replicating the results for movers alone), respondents are also more accurate when they have children in school and when they have higher incomes.

Repeating the sensitivity analysis, across the entire sample, the probability of having accurate information is 24%. But if all respondents had no experience living in the community, accuracy would fall to 19%. Similarly, if all respondents had children in schools, accuracy would increase to 30%. And if all respondents had high income, accuracy would increase to 29%. Combining these two conditions for all respondents increases the accuracy rate to 37%—165% higher than chance.

To illustrate the search and experience dimensions of local public goods in visual form for both recent movers and established residents, Figure 2 compares the increasing effect of length of residence on the accuracy of information (the experience dimension) with the accuracy of recent movers divided into those with low income and high incomes. The graph illustrates that high-income movers treat local public goods as a search good by gathering accurate comparative information while they are considering a housing purchase. For nonmovers there is a clear linear trend relating time in a community to accuracy about schools.[13]

Our survey evidence also illuminates the search processes used by different types of movers. Compared to low-income movers, movers with higher resources are significantly more likely to use "institutional sources," such as schools and governments, to provide them with information. People with lower incomes are more likely (at $p < .07$) to rely on "personal sources," especially friends and family for information about local services. (For parallel evidence from private markets, see Slama and Tashchian 1985, 72.) More importantly, movers who report caring more about local government services are more likely to use both kinds of sources to gather information than are other respondents (significant at $p < .06$).

Incorporating the Role of Voice. One criticism of the Tiebout model is its failure to identify the importance of voice as a driving force in the local market for public goods. We believe that voice reinforces the pivotal role of the high-income marginal consumer. Inman (1978) found that a group virtually identical to our informed consumers (younger, high-income households with children), even though making up a small percentage of the population, were likely to have their preferences for school budgets met. They achieved their success by exercising voice, that is, through their active participation in the local political process.

The knowledgeable mover will be more readily able to make comparisons between services and taxes in competing jurisdictions and will have solid evidence upon which to base complaints about the services currently provided. Paralleling Inman, we find that better-informed movers are also significantly more likely to have engaged in higher levels of political activity.[14] These individuals combine entry and voice to increase their power as consumers over the behavior of municipal agencies.

TIEBOUT REDUX

In the last decade, many scholars have attacked the Tiebout model by challenging the accuracy of the individual-level assumptions that they believe underlie the model. These critics argue that citizens with incomplete information cannot force a local market for public goods to operate competitively, regardless of how many alternative suppliers are available in a metropolitan area. This work ignores recent economic models and marketing studies that analyze competitiveness in markets with incomplete information. In contrast, we believe that it is possible to construct a model of the local market for public goods that begins to resolve the apparent macro–micro disparity.

Just as the theoretical ideal of perfect competition in private-sector markets is never found in the real world, Tiebout established an abstract ideal that he recognized was "an extreme model" (1956, 419). He further warned that "those who are tempted to compare this model with the competitive private model may be disappointed. Those who compare the reality described by this model with the reality of the competitive model—given the degree of monopoly, friction, and so forth—*may* find that local government represents a sector where the allocation of public goods (as a reflection of the preferences of the population) need not take a back seat to the private sector" (p. 424, emphasis original).

In the development of their theories, economists studying industrial organization relax extreme assumptions about perfect competition to test the robustness of the actual markets they analyze. Similarly, we have examined some of the extreme assumptions of the Tiebout model that have been often criticized. While our work moves away from Tiebout's precise abstract model, we provide a more realistic micro foundation for the most important macro outcome of the model, namely, that the presence of multiple local governments creates pressure toward efficient outcomes in the local market for public goods despite widespread ignorance on the part of individuals about local services and taxes.

Our results show that many established residents are ignorant of local services and taxes. For most of these residents, ignorance is "rational," since they are not making any relevant decisions based upon these issues. Taken as a whole, the average citizen–consumers consider local services and taxes unimportant and not surprisingly, have information about local school taxes and expenditures scarcely more accurate than one would expect by chance. Does this mean that there are no competitive forces in the local market for public goods and that "the aggregate-level findings on service delivery may have to be rethought" (Lowery and Lyons 1989, 95).

We believe that rejecting the Tiebout model of the competitive local market for public goods on this basis is wrong. Even in highly competitive private markets, many consumers do not have accurate comparative information about product quality (Lichtenstein and Burton 1989). But economic theory suggests that it is not necessary for *all* consumers to be informed about choices in order for a market to move toward a competitive equilibrium. These theories suggest that if a subset of consumers make accurate comparisons and act on that knowledge, it may be sufficient to make a market behave competitively. Most importantly, when these consumers are the most desirable segment of the market, the pressure toward competitive outcomes is even greater.

High-income movers have more accurate information about their schools and those who report they care about local schools are also more accurate about local public services and taxes. These important households gather accurate comparative information and they act upon this information when shopping around before entering a community. But even more important for understanding the local market for public goods, these citizens are the very people that communities have the strongest incentives to attract. As communities seek to attract these higher-income individuals, the resulting competition may benefit all citizens, including nonmovers.

Are there enough of these people to make a market? Or, in more general terms, how many consumers must have accurate information for a market, particularly a market for local public goods, to be pushed toward a competitive outcome such as Tiebout described? Wilde and Schwartz lament the absence of rigorous analytical tools to answer the question (1979, 543). While surprisingly little systematic attention has been devoted to the issue in the economics literature, some observers have offered the view that very few informed consumers can make a market. In a hearing on the Truth-in-Lending Law, Senator Paul Douglas once remarked that a competitive market would exist if "only" 10% of the consumers were cost-conscious (ibid.). Empirical studies have shown that Douglas's impression was really not that far off the mark. Thorelli and Engledow argue that "information seekers" make up only 10–20% of the population (1980, 14). Claxton, Fry, and Portis (1974) showed that only 5–8% of furniture and appliance shoppers had extensive information about their purchases. And Inman (1978) showed that a highly informed set of voters, making up about 13% of the population, was able to make local school budgets respond to their preferences.

The precise percentage of informed consumers needed is likely to vary according to the price of the good, the ability of suppliers to price-discriminate among consumers, the number of alternative suppliers in the market, and the strategic actions available to suppliers. But the purchase of a house and the local public goods that are bundled with that house favor the ability of a subset of consumers to drive a competitive market.

Dagger (1981) estimates conservatively that 20% of Americans change their local jurisdictions over a five-year period. Since the upper-income half of our mover sample hold much more accurate comparative information than other movers, over a five-year period, perhaps 10% of the population in an average community will have recently entered the community and have high levels of comparative knowledge of local public goods. We believe that this is enough to put pressure on communities to be competitive in their services, especially since these are the residents most important for the continued fiscal viability of communities.

Clearly, while informed entry helps drive a local market for public goods in something approximating Tiebout's "pure theory," other actions also stimulate competition that forces governments to provide services efficiently. Governments, especially those in polycentric regions, try to attract desirable businesses by keeping their taxes low in relation to the services they deliver. Businesses have strong incentives to make well-informed entry choices, reinforcing the concerns of high-income movers. Nonmovers can also use local politics to change service delivery patterns, and they are often assisted by "political entrepreneurs" who aggregate their collective "voice" (see Schneider and Teske 1992).

As Thorelli and Engledow (1980) noted for information-seeking consumers in the private market, Sharp (1984, 1986) for local public goods, and Hirschman (1970) most generally, mobility and voice are related. Citizens with the most accurate knowledge of local public goods are also more likely to be politically active. This is clearly true for high-income movers. But it is also true for nonmovers: based on the comparative knowledge that they acquire over time, they use voice to improve the service–tax package in their community.

While entry pressures communities to be competitive, the combination of entry and voice is likely to provide even more effective pressure on local governments, providing the mechanisms that make competitive models of local governments work. But it is only by understanding how these micro political behaviors interact with macro market mechanisms that we can more accurately specify the competitive local market for public goods.

APPENDIX A

As economists have recognized that few markets fulfill assumptions of complete information and perfect competition, they have increasingly focused on the role of information in markets. Complete information is too costly to generate in most transactions, and no party ever has complete information about a product or service. Stigler (1961), Nelson (1970), and Ackerlof (1970) produced the seminal works in the area of information economics. Subsequent to these explorations, economists have done theoretical work on price–quality relationships, on the elements of search and experience that allow consumers to learn about purchases, and on how competitive markets behave in the absence of full information by all consumers. More directly important for our purposes, they have also studied the effects of search processes and market equilibria and identified two basic types of search: sequential and fixed sample (Cave 1985). While there are important differences in the equilibria expected from either of these broad search processes, under either approach, more extensive search and greater numbers of informed consumers in any market produce stronger pressure toward competitive outcomes.

Bucklin summarizes the critical processes: "Fundamental to the efficient operation of a competitively organized market economy is the function of consumer search. If buyers fail to seek out alternative prices and qualities and thereby become ill informed, markets become segmented, prices rise, and consumer surpluses are transferred to the entrepreneurial sector" (1969, 416).

Unfortunately, economists have done little empirical work on these questions. However, their more applied colleagues in business, marketing, and consumer research have provided some evidence in real markets, which we summarize here.

In the American economy, where many markets are competitive, much of the research into private goods markets shows that many or most consumers do not comparison-shop very much. For example, Claxton, Fry, and Portis (1974) found only 5% of furniture buyers and 8% of appliance buyers to be store-intensive, thorough shoppers, with an average of 20 store visits for furniture shoppers, compared to 8 or less for other shoppers. They found that these informed subsets of shoppers utilized more sources of information and had higher incomes. For the other buyers, they argue that "a basic conclusion from available studies is that pre-purchase search is a relatively limited activity, even in the case of major durables" (p. 35). Earlier, Katona and Mueller (1955) found that 47% of those purchasing durables bought at the only store they visited, a percentage that jumps to 70% for nondurables. Thus, most consumers do not engage in extensive comparative shopping for products that are sold in what we would nonetheless consider competitive markets.

Other studies also find a group of consumers that search more than others. These consumers are more interested in and "involved with" the product, and they usually have higher incomes (Claxton, Fry, and Portis 1974; Katona and Mueller 1955; Newman and Staelin 1972; Slama and Tashchian 1985). For example, the National Commission on Consumer Finance reported that "the number of people unaware of the Annual Percentage Rate paid on credit for recent purchases ranged from almost 90 percent in poverty areas to 52 percent in market populated by relatively affluent buyers" (Wilde and Schwartz 1979, 543).

Two sets of studies in particular have focused on the critical importance of consumers who search more (and presumably are therefore better informed about comparative price and quality information). Feick and Price (1987) labeled the upper third of information seekers *market mavens*, who are likely to be opinion leaders and to share comparative information with other, less-informed consumers. Slama and Williams (1990) generalize Feick and Price's findings to more markets and confirm that market mavens provide comparative product information to others for many products and services, including real estate.

In a second line of research, based on a major cross-national study over many years and focusing on users of consumer information (such as *Consumer Reports*), Thorelli and Engledow (1980) believe that a category of *information seekers* are significantly more interested and more knowledgeable than the *average consumer*. They argue explicitly that these information seekers, who make up 10–20% of the population and are of higher income than the average consumer, help police the market by comparative shopping and by complaining more (e.g., using "voice," in our parlance) if products and services are not adequate:

> The test report magazine subscribers represent a fairly homogeneous and cosmopolitan group of information-sensitive consumers, which may be found in all industrially advanced countries, and which is significantly different from the average consumer group. [We] estimate that from 10 percent to 20 percent of the consumers in a high consumption economy may be [information seekers]. They are both affluent and consumption-oriented, so that they account for a large number of total purchases and a large dollar volume . . . in their role of opinion-leaders and vigilantes of the marketplace, the [information seekers] constitute a public good. (pp. 12–14)

Thus, economic and consumer evidence supports the notion of a cadre of high-income consumers helping to make markets more efficient, even in the face of large numbers of nonsearching consumers.

APPENDIX B

For the survey of nonmoving householders, we generated a telephone-number sampling pool using listed telephone numbers and the "add-a-digit" approach (Lavrakas 1987, 39–41). This survey was conducted in October and November of 1991 and the interviewers were undergraduate students participating for class credit. The overall response rate was 49%. To obtain this response rate, up to a total of nine callbacks were made to every selected telephone number. Households with listed telephone numbers received an advanced letter informing them of their selection in the study. All selected individuals who refused on the first call were recontacted by an experienced interviewer. The response rate was calculated as the ratio of completions to the sum of completions, refusals, and numbers at which there was no answer. Of the 310 completed interviews, only the 239 cases in which the residents owned and had been living in the present residence for at least a year were included in the subsequent analysis.

For Suffolk residents who had recently bought houses, a more complicated procedure was used to generate the telephone-number sampling pool. We obtained lists containing all the names and addresses of recent purchasers (plus the price of the house) as recorded at the Suffolk County Clerk's Office during October 1991. Telephone numbers were then matched with the listed names and addresses. The questionnaire contained items with wording identical to that used in the survey of established residents. These questions were augmented with others having to do with matters such as reasons for moving and the nature of the search undertaken. This survey was conducted in January 1992 using the best interviewers from the previous survey. The overall response rate was 54%, resulting in 259 completed interviews.

Both samples achieved a fairly even mix of the sexes. (The sample of established residents had 44% male and 56% female respondents, while the sample of recent movers had 53% male and 47% female.) Our sample of established residents had lived in their current homes for an average of 18 years. Aside from this difference in length of residency, the demographics of the established residents and recent movers were quite similar. The majority of respondents were white (93%) and well educated (37%, some college, 39% a college degree).

The average age of the established residents was 46 years; the average for recent movers was 35 years. Reflecting this difference, established residents were more likely to have children; and 33% of them had children in school, compared with 24% for recent movers. However, recent movers were more likely to expect children in future (88% compared with 62%).

Compared to national figures but reflecting the demographics of the county, the respondents in both samples were better educated and had slightly higher income than the general population. According to the 1991 *Statistical Abstract of the United States*, 17% of the population in 1989 had some college, and 21% percent had completed a college degree. In the same year, the median household income was $34,000. At $157,000, the average price of the houses purchased by the recent movers was also somewhat higher than the comparable national figure. The median sale price in 1989 of a single-family home in the United States was $120,000 (Schwenk 1991).

In 1990 (the most recently available figures at the time of the survey), expenditures per student in Suffolk County ranged from $7,107 for the 9,225 students in the William Floyd School District to $43,048 for the 43 students in the Fire Island District. The highest expenditure per student for districts with over 100 pupils was $16,051 for the 1,940 students in the Shoreham–Wading River District. Average school expenditure per student in Suffolk County was $9,664. These figures are high even for the northeastern states and almost twice the average school expenditure per student for the nation as a whole. The 1992 *Almanac of the Fifty States* puts the average expenditure per pupil in the United States in 1990 at $4,890.

Notes

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1. Schneider (1989) explores other problems with applying a model of the private market to the local market for public goods.

2. The Tiebout model and its extensions have also been criticized on equity grounds. Hill (1974) described the polycentric system of metropolitan government as "separate and unequal." We are concerned here only with the efficiency aspect of the model.

3. In addition to our central concern here, the Lowery and Lyons approach can be criticized on several other grounds. For example, Percy and Hawkins (1992) note that counties, not local government, are often perceived as the most important service deliverers in the south (where Lowery and Lyons did their analysis) but not in the rest the nation. Percy and Hawkins (1992) and Sharp (1986) find that citizens in the Milwaukee and Kansas City metropolitan areas, respectively, are affected by service–tax packages. Both argue that they find evidence of Tiebout market mechanisms. Furthermore, users of economic models prefer to focus on revealed preferences as demonstrated by actual individual choices, not on survey responses that have no action component. For example, while surveys of New York City residents routinely find that more than half of them say they are planning to move from the city soon, half the population does not leave. Thus, actual, not potential, movement seems more relevant, and Lowery and Lyons do not measure actual exit. Finally, schools are the most important and expensive local public good provided in most jurisdictions and Lowery and Lyons' analysis excluded schools because both of their metropolitan areas have consolidated school districts.

4. For example, only 30% of buyers of nondurable products shop at more than one store before making their purchases (Katona and Mueller 1955); for larger items, Claxton, Fry, and Portis (1974) found that only 5% of furniture buyers and 8% of appliance buyers gathered extensive information.

5. This may reflect the influence on political science of Hirschman's (1970) seminal work, *Exit, Voice, and Loyalty*. The course of the debate might have been different, if Hirschman had included *Entry* in his title.

6. We use the term *municipalities* broadly. The delivery of local services is accomplished by a variety of government agencies. However, we believe that in the Tiebout model, any agency that delivers services and levies taxes to pay for those services will be subject to Tiebout-like pressure. This includes general purpose municipalities but it also includes special districts, including the "municipal" agency in charge of the most costly local service delivered, that is, *school districts*, which deliver *public education*.

7. While economists do not usually try to isolate particular individuals at a precise point on a market demand curve,

following Hirschman (1970), we do this to highlight the difference between the average and the marginal consumer. We also recognize that for private goods, market demand is a horizontal summation of individual demands, while for pure public goods it is a vertical summation. This distinction does not affect the overall thrust of our illustration.

8. We focus on schools because almost two-thirds of local property taxes in Suffolk County are collected by school districts. Moreover, there is substantial variation in the taxes and expenditures across the 71 school districts that make up the market for education. We also examined information levels about parks and roads, services which are provided mostly by the 10 towns in the county. Multinomial probit analyses were conducted to explain variation in a scale representing respondent's combined knowledge about school district taxes, town taxes, and expenditures on schools, parks, and roads. This analysis essentially replicated the results reported for schools and is available on request. We report the results only for schools in the interests of simplifying an already complex argument and because our expenditure data for schools are more accurate and finer-grained than the other expenditure data. See Appendix B for information about Long Island.

9. To test the accuracy of information, we asked our respondents a series of questions about the taxes and expenditures of their local school district—the most important and most expensive local service. Respondents were asked the following question: "Compared to all school districts in Suffolk County, would you say that the amount spent on each student in your school district is: above average, below average, or about average." Respondents could also offer "don't know" as a response. A similar question was asked for taxes. Respondents who said they did not know their expenditures or taxes were automatically classified as holding wrong information. To be judged as having correct information, a respondent had to choose the branch that correctly characterized their school district. Thus a respondent who said local school expenditures (taxes) were above (below) average had to live in a school district whose expenditures were actually above (below) average. Those who responded "average" were considered accurate if they were in one of the 50% of districts surrounding the mean. Thus a respondent who gave any response other than "don't know," had a 25% chance of taking any of the four paths and a 50% chance of being correct. We therefore expect a respondent to get the expenditure or tax question correct 37.5% of the time and both right $.375 * .375 = 14\%$, by chance.

10. The figure for established residents is biased upward somewhat because we eliminated residents who had lived in their current community for less than one year, and increasing length of residence is associated with greater accuracy.

11. Respondents were read a series of categories of increasing income levels and asked to stop the interviewer when the category was reached that encompassed the respondent's household income. The "average" category for respondents was forty-to-fifty thousand dollars. We therefore define upper-income individuals as those respondents with incomes above fifty thousand dollars. For movers, we duplicated every analysis presented using house price. Given that local taxes are overwhelmingly based on property values, house values may be a more accurate reflection of the resources municipalities really care about. We report the results for income, rather than house price, because accurate house price information was not available for nonmovers. Moreover, there is a high correlation between income and house price and the results based on house price are virtually identical to the results reported.

12. This difference between income groups is significant at $p < .0001$. Furthermore, 28% is significantly better than the 14% accuracy expected by chance, while 12% is not significantly different from chance. For comparative purposes, note that among established residents, income has a similar effect, increasing the accuracy rate from 22% for low-income residents to 28% for high-income residents.

13. The trend line in Figure 2 is the regression line plotting accuracy against the square root of the length of time a respondent has lived in a community. We used the square root because the distribution of the variable is highly skewed, with a handful of respondents having lived in the same community for over 50 years.

14. In a series of questions, respondents were asked, Have you ever attended a meeting called to discuss problems in your neighborhood or local community? Joined an organization attempting to solve these problems? Signed a petition regarding any particular local problem? Telephoned or written to an elected official regarding local problems? Joined a group for parents of school students like a parent–teacher organization? Voted on a school budget? We summed responses to create a political activity scale running from 0 to 6. High-income movers reported a mean of 1.7 political activities, significantly higher than the 1.2 average for low-income movers ($p < .04$). Movers who volunteered that local services were important in their choice of community engaged in an average of 1.8 activities, significantly higher than the 1.28 activities engaged in by movers who did not say that local services affected their choice of community ($p < .02$).

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RESEARCH
NOTE

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LAWYERS, ORGANIZED INTERESTS, AND THE LAW OF OBSCENITY: AGENDA SETTING IN THE SUPREME COURT

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Each year thousands of cases and litigants come to the Supreme Court. How can the Court find the most important cases to decide? The law of obscenity illustrates particularly well the Court's problem as it constructs its plenary agenda. Using data drawn from petitions for certiorari and jurisdictional statements filed with the Supreme Court from 1955 to 1987, we formulate and test a model of case selection in which professional obscenity lawyers and organized interests figure as critical elements in the process of agenda building. We also encounter strong evidence of the Court's differential treatment of several different litigants. Moreover, the calculus of selection changed markedly over time, as the Court itself changed; the Burger Court and Warren Court weighed several of the criteria quite differently.

From thousands of candidates each term, the Supreme Court chooses one hundred or so cases for plenary review. This is one of the most important facets of the Court's job in the political process, for its agenda reflects choices on policy. And case selection is not a simple matter. The Court must look not only for appropriate legal vehicles and likely targets for reversal but also for cases of political, social, and economic import. A great deal is known about how the Court, in general, selects cases (see, e.g., Caldeira and Wright 1988, 1990; Perry 1991; Ulmer 1984)—but next to nothing about how this winnowing takes place in particular areas of law. We shall analyze litigation in the law of obscenity, a relatively well defined area. This set of cases permits us to illustrate the generic problems of agenda building in a richer context—the interplay of policy and doctrine, factual situations, and combinations of litigants. The multitude of peculiar, often bizarre sets of facts and conflicting legal assertions associated with this area magnifies the Court's more general problem of locating and utilizing reliable information in the development of a plenary docket and makes it an especially good target for research on agenda setting.

Friends of the court constitute one credible source of assistance in this process. Caldeira and Wright (1988, 1990) have demonstrated the force of organized third parties in the formation of the Court's agenda. We build on that theoretical and evidentiary base but weave an additional thread into the fabric—the role of the bar in these choices. We set forth a theoretical framework in which both organized interests and professional obscenity litigators figure prominently in the selection of cases. To create its agenda of cases in the law of obscene expression, the Court relies not only on the briefs of third parties but also on the presence or absence of members of the libertarian "obscenity bar" (a set of litigators experienced in these issues) to draw its attention to the cases of potentially broad impact on public policy. We ask the questions, Do experienced litigators make a difference, above and beyond the usual considerations? If so, how and why? Did organized interests as amici increase the chances of cases? Did the Supreme Court respond differentially to the claims of different classes of litigants? Did the calculus of the Court in the law of obscenity shift as the membership turned over and the Burger Court emerged?

THE CONTEXT OF OBSCENITY LITIGATION

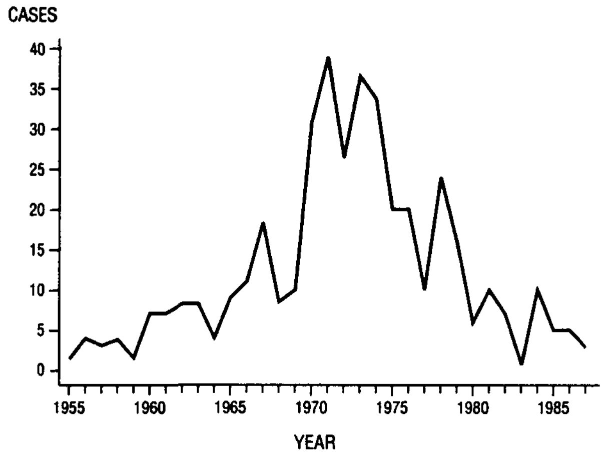
Deciding what constitutes obscene expression and by what means the state may legitimately control it has been the source of considerable frustration for the justices. The difficulty associated with that process is reflected, at least in part, in the Court's jagged jurisprudence in this area (Kalven 1960; Krislov 1968; Lockhart and McClure 1954; Magrath 1966; Schauer 1976; Stone et al. 1986; Tribe 1988). Finding the most appropriate cases for resolving these issues has proved equally problematic for the Court, since so many of the petitions, while advancing broad constitutional claims, are punctuated by peculiar erotica. Obscenity, therefore, provides a useful setting for exploring how lawyers and organized interests assist the Court in the complex process of locating issues of true national consequence.

Figure 1 provides a traceline of filings on obscenity in the Supreme Court from 1955 through 1987. By our definition of obscenity, litigants filed 410 cases in this period. This number includes petitions for certiorari and writs of appeal from both libertarian claimants and governmental agencies. Prior to and immediately after the Court's initial ruling in *Roth* v. *United States* (1957), libertarians filed only a handful of cases each term. *Roth*, however, apparently invited further litigation, as the number of filings began a secular trend upward, albeit around a couple of valleys.

Opinions among the justices on the appropriate

FIGURE 1

Obscenity Petitions before the Supreme Court, 1955–87



standards for obscene expression soon became fractured. For several years, beginning in *Redrup* v. *New York* (1967), the Court gave up altogether the quest for a standard and adopted the practice of *per curiam* reversals of convictions for the sale or exhibition of materials if five or more members, using various tests, did not view the materials in question as obscene. A good many emboldened libertarians of one sort or another responded in kind, petitioning the Court with greater frequency. Lawyers for libertarian losers in the lower courts, under *Redrup*, had little to lose and much to gain from a try in the Supreme Court.

Enter the Burger Court. In *Miller* v. *California* (1973), the Court abandoned the practice of *Redrup* and a majority adopted a new test, one that clearly took a much dimmer view of obscene matter than had the Warren Court (Lockhart 1975; Schauer 1979). From that point, we see a secular decline in obscenity filings; and this actually understates the proportional decline, since the caseload of the Court was much smaller in the 1950s and 1960s. For several years after *Miller*, the Court maintained a heavy dose of obscenity cases on its docket, as libertarians and proscriptionist litigants explored the limits of the doctrine of *Miller*. By the 1980s, however, the flow of cases had subsided. All of these numbers suggest in broad terms a connection between the actions of the Court as a policymaker and the choices of litigants in the courts below regarding whether to seek review in the Supreme Court. Many of those choices were shaped by organized interests and professional litigators, actors who have had a major hand in the development of the law of obscenity in the Supreme Court.

From the mid-1950s to the present, obscenity litigation has provided fertile ground for organized efforts of various sizes and kinds.[1] Twenty-four organizations participated, in one guise or another, in support of the libertarian position during the period of our research. Some of the organizations represented "commercial" interests, such as the American Booksellers Association and the Adult Film Association of America; others stood up for professional and occupational interests, such as the Authors League and the American Library Association. One or more of these organizations participated, in some fashion or another, in more than a half of the obscenity cases decided on the merits in the Court.

In contrast, organized interests filed a brief amicus curiae prior to certiorari or jurisdiction in only 24 of the 410 cases in our sample. This number seems small, but it actually compares favorably with the rate of briefs amicus curiae filed prior to certiorari or jurisdiction in a recent term of the Court (Caldeira and Wright 1988, 1990). Nevertheless, the contrast between the density of participation in agenda setting and the decision on the merits strikes us as noteworthy. Participation prior to certiorari or jurisdiction entails greater costs and has a higher threshold. Of the 24 organizations that filed a brief amicus curiae on the merits, only a handful filed one or more during the formation of the Court's plenary agenda. Most of these organizations filed or joined only a single brief prior to certiorari or jurisdiction but several did participate on multiple occasions. Among those endorsing the libertarian interests were the American Civil Liberties Union, the Authors League, the American Book Publishers Council, the Council for Periodical Distributors Association, and the National Association of Theatre Owners. On the proscriptionist side, the Citizens for Decency through Law and the State of Georgia took part as amici prior to the merits.

Over the years, many lawyers for libertarian clients gained experience, expertise, and reputation. These private lawyers and law firms soon constituted an "obscenity bar"—a set of practitioners who regularly litigated questions of obscene expression and who gained prominence as defenders of libertarian positions and people. Indeed, by the mid-1970s, these lawyers had founded a professional organization, the First Amendment Lawyers Association, to which some 125 practitioners belonged. By our counting, members of the obscenity bar appeared for a petitioner in substantially more than half of the cases filed in the Court from 1955 through 1987. The obscenity bar's extraordinary rate of participation as direct representatives of libertarian claimants helps to make sense of the relatively low density of organized efforts as amici curiae prior to the decision on the merits. Obscenity lawyers can serve as the functional equivalents of interest groups in experience, access to information, and reputation in the Supreme Court (see McGuire 1993).

In short, despite the frivolous nature of many of the cases in this issue area, litigation on obscenity has featured an environment rich with organizational participation and sophisticated litigators who have served to articulate significant concerns to the members of the Supreme Court. What role, then, did these interest groups and the appellate practitioners play in the process of agenda building?

THEORETICAL FRAMEWORK

We assume a set of justices who prefer some policies on the restriction or protection of sexual expression over others on ideological or other grounds, who normally seek to implement these goals and do so by selecting a set of cases with the greatest potential consequences for the direction of the law of obscenity. Normally, the ideological values in collision will stand out in a case, and the members of the Court, or the law clerks, will require no subtle analysis of the political consequences of a dispute. The law of obscenity, however, is quite a different matter. A cursory glance at the summary of the argument in the petitioner's brief (or, in the more sensational cases, the name of the petitioner) will readily communicate the nature of the ideological conflict. It is a good deal more difficult to discern the social, political, economic, or legal importance of a case, given the general lack of classic indicators of cert-worthiness, such as conflict between circuits or the participation of the United States (see Baker 1984; Frey, Geller, and Harris 1987), as well as the dubious nature of many of the claims advanced by liberal petitioners. The Court, therefore, often lacks a solid lodestone for the selection of cases in this field. How then does the Court determine the "importance" of a case when the issues are apparently trivial at best and when the traditional signposts of cert are absent?

Members of the Court use the participation of organized interests as amici curiae prior to certiorari or jurisdiction as an indicator of the importance of a case among those other than the immediate parties (Caldeira and Wright 1988, 1990; Ennis 1984; Perry 1991; Spence 1974).[2] The participation of an obscenity litigator as a direct representative of a libertarian petitioner sends another strong signal of the importance of a case. The mere presence of a repeat player as counsel goes a long way toward demonstrating the seriousness of the petitioners. Few practitioners file more than one petition for certiorari or argue more than one case in a life time. Thus experience is a valuable and valued commodity among appellate lawyers. Members of the "obscenity bar" acquired experience by taking part in litigation on obscenity as a large and regular part of their business. These lawyers should enjoy the advantages of "repeat players" (Galanter 1974).

If an organized interest planned to participate in only one case, it might have an incentive to communicate inaccurate information about the importance of a case to the Supreme Court. For, after all, organized interests and the Supreme Court do not have the same goals, interests, and values. Organized interests initially possess more information about a case than does the Supreme Court, and the reliability of organized interests is not easily discerned in the short run. But organized interests participate in the Court repeatedly and develop reputations among the justices (see, generally, Kreps 1990; Spence 1974). Justices may check the recommendations and assertions of an organized interest against the outcomes of agenda setting: experience then translates into reputation. The repetitive nature of these interactions lowers the costs of monitoring for the Court. As a consequence of reputation, not all organized interests will carry the same weight. The Court rewards with attention those who repeatedly demonstrate credibility, and payoffs of attention for faithful claims of importance reinforce truthfulness (Ennis 1984).

Much the same logic applies to lawyers before the Court. Lawyers seek to gain access to the Supreme Court to win a lawsuit for clients, and this no doubt provides a strong incentive to exaggerate the importance of a case. To be sure, lawyers are "officers of the court," but the strong divergence of interest reduces their credibility as honest brokers of the importance of a case. Lawyers, after all, must deal with multiple actors, including clients, the Court, and partners. For a lawyer who files a petition or jurisdictional statement only once in his or her professional life, it is sensible to misrepresent the importance of a case. Cheating has low costs; monitoring costs run high; and if the Court believes the claim, counsel may obtain a plenary review. Of course, members of the Court are well aware of the incentives for "one-shotters" in the formation of the agenda. Repetitive participations permit the Court to observe the reliability of a lawyer's legal and factual claims. Those who bring cases to the Court over and over again build up a reputation in the eyes of the Court, a costly and valuable asset (McGuire 1993). Reputation not only helps to attract and maintain clients but also translates into influence in the Court. Thus, lawyers and other repeat players have a strong interest in maintaining credibility in the Court. To discover the importance of cases, then, the Court takes advantage of the self-interested behavior of amici and professional litigators.

The incentives of professional obscenity litigators make them less reliable than amici curiae as sources on the importance or quality of a case. Importance of a case represents only one among many reasons a lawyer chooses to take a case to the Court. For example, a client may wish to delay a final judgment such as a large financial payment. Lawyers normally do not have the luxury of selecting the ideal vehicle from a large pool. In contrast, amici curiae can survey the cases filed in the Supreme Court and support those best suited to plenary consideration. And amici curiae need not take into account the interests or concerns of a client. (For more on these differences, see Caldeira and Wright 1989.)

These considerations lead us to several hypotheses. The appearance of a professional obscenity lawyer for a petitioner or the support of an organized interest as amicus curiae should increase the probability of the Supreme Court's granting a particular case. One or the other form of participation should provide the signal required to alert the Court of the importance of the case. Thus, in light of the structure of incentives, the Supreme Court should weigh the participation of an amicus curiae more heavily than the presence of a professional obscenity lawyer as counsel for a petitioner.

Competing Explanations

The presence of a professional obscenity litigator and amici curiae in support of the petitioner are, of course, only two of many considerations in the calculus of the Court. Prior research points to a wide range of other forces at work (Armstrong and Johnson 1982; Brenner 1979; Caldeira and Wright 1988, 1990; Perry 1991; Provine 1980; Songer 1979; Tanenhaus et al., 1963; Teger and Kosinski 1980; Ulmer 1978, 1983, 1984). Variables in play might include the presence or absence of "conflict" between or among lower courts or the allegation of such a conflict; the route by which a petitioner brings a case, since the Court has tended to review writs of appeal at a higher rate than writs of certiorari; disagreement between the lower courts; dissent in the court below; the nature of the issue area; and the ideological direction of the outcome in the federal or state appellate court.

Quite apart from these indices, the Court also accords parties differential treatment, at times favoring "underdogs" and at other times favoring "upperdogs" (Caldeira and Wright 1989; Ulmer 1978). The solicitor general of the United States does much better than any other party in gaining access to and victories on the merits in the Supreme Court (Caldeira and Wright 1988; Caplan 1987; Segal 1988; Segal and Reedy 1988). By contrast, state and localities do much worse. Traditionally, the regulation of obscenity has fallen to state and local governments under the "police powers" of the states. We might, therefore, expect the Court to recognize the unique needs of individual states in abating pornographic expression and to give careful attention to their petitions for plenary consideration of losses in the lower courts. To be sure, until the last few years, state governments have fared poorly in the Court in part because of poor representation and in part because of the ideological predispositions of the Court (Perry 1991, 127; but see also Epstein and O'Connor 1988). We might also expect local governments to receive disproportionate access to the Supreme Court. Local governments are arguably best equipped to judge whether certain materials offend community standards, but they are also the most likely to engage in excesses. Communities can enact ordinances with relative ease; and local majorities may tyrannize otherwise legitimate, protected expression.

Some of the libertarian petitioners are respectable individuals and organizations; others, specialists in pornographic materials. The nature of the parties might well reflect on the credibility of the petitioners. It is plausible to imagine the Court taking a dim view of pornographers—just as, for example, Justice Marshall has expressed wariness of drug dealers. We consider four broad categories of libertarian petitioners.[3]

Corporations generally receive favorable treatment in litigation but do not fare so well in the formation of the Court's agenda (Caldeira and Wright 1989; Galanter 1974; see also Sheehan, Mishler, and Songer 1992). Parties such as magazine distributors or motion picture companies are probably the most respectable of the libertarian petitioners in litigation on obscenity. Because these parties often deal in a wide range of materials, they are arguably less likely to be perceived as pornographers. Parties such as bookstores and theaters, however, in litigation on obscenity, usually deal in a narrow range of sexually explicit materials. These are generally "adult" bookstores and theaters. We would not expect these parties to find favor in the Supreme Court. Individuals figure as petitioners in a number of cases. These cases involve a conviction for a violation of a regulation of obscenity, including the distribution of obscene materials, transportation of them across state lines, or attempts to import them into the United States. Individuals generally do not do well in the Supreme Court (Caldeira and Wright 1989), and the association with the purveying of sexually explicit materials should not help.

The Court, as our model implies, pursues policy objectives as well as legal criteria in composing the plenary agenda. Thus, the responses of the justices to legal arguments, stipulated and contested facts, and the severity of the regulation in question in large part reflect the policy preferences of the Court.

Governmental actions with the potential to infringe upon freedom of expression receive particularly close scrutiny from the Court. In general, we would expect the Court to examine more carefully the most stringent forms of regulation of obscenity. Governments have devised a number of ways of dealing with obscenity, some benign, others posing a significant threat to legitimate expression. Prosecution for violation of a valid law is the most innocuous of these methods.

Injunctions against the distribution of obscene materials may pose a serious threat to freedom of expression (e.g., *Vance* v. *Universal Amusement Company* 1980). Of course, injunctions do not issue without the independent judgment of a third party, but there is clearly a possibility of a prior restraint of expression. Searches and seizures are not always conducted with the sanction of a magistrate. Thus, the seizure of allegedly obscene materials may encroach upon libertarian rights (e.g., *Marcus* v. *Search Warrant* 1961), and licensing raises grave constitutional dangers (e.g., *Freedman* v. *Maryland* 1965). Governmental schemes for clearing proposed communications effectively place in the hands of officeholders a means of exercising censorship of the free flow of ideas. Accordingly, we expect the Court to grant a review in more of these cases than in others.

Legal claims might also raise the salience of a case. Other things being equal, a claimed violation of the First Amendment should raise the probability of a plenary grant. Most, but not all, of the petitioners in our sample claimed a violation of the First Amendment (72%).[4] Petitioners or appellants claimed "redeeming literary value" for the materials in question in 35% of the cases. Thus, the materials may be sexually explicit, but the petitioners claim legal pro-

TABLE 1

Probit Coefficients for Plenary Review in Law of Obscenity

VARIABLE	COEFFICIENT	STANDARD ERROR	T-STATISTIC
Intercept	−.71	.70	−1.01
Professional obscenity lawyer	.97	.39	2.49*
Brief amicus curiae	1.45	.60	2.42*
State government as petitioner	−.70	1.02	−.69
Bookstore as petitioner	−.62	.46	−1.35
Theater as petitioner	−2.62	.87	−3.01**
Individual as petitioner	−1.27	.62	−2.05*
Corporation as petitioner	−.53	.52	−1.02
Severity of regulation	−.07	.20	−.35
First Amendment	.51	.55	.93
Literary value	1.75	.41	4.27***
Writ of appeal	1.07	.43	2.49*
Alleged conflict	−.71	.45	−1.58
Reversal between lower courts	−.93	.94	−.99
Burger Court	−.90	.82	−1.10
Professional obscenity lawyer × Burger Court	−.67	.47	−1.43
Brief amicus curiae × Burger Court	.12	.74	.16
Solicitor general as petitioner × Burger Court	1.99	.62	3.21**
State government as petitioner × Burger Court	1.49	1.14	1.31
Local government as petitioner × Burger Court	.22	.54	.41
Bookstore as petitioner × Burger Court	.25	.53	.47
Theater as petitioner × Burger Court	2.65	.91	2.91**
Individual as petitioner × Burger Court	1.10	.72	1.53
Corporation as petitioner × Burger Court	.34	.60	.57
Severity of regulation × Burger Court	.18	.24	.75
First Amendment × Burger Court	−.61	.66	−.92
Literary value × Burger Court	−1.11	.48	−2.31*
Writ of appeal × Burger Court	−.57	.49	−1.16
Alleged conflict × Burger Court	.74	.52	1.42
Reversal between lower courts × Burger Court	1.12	.99	1.13

Note: N = 410 (95 grants, 315 denials); dependent variable equals 1 where the justices granted certiorari or noted probable jurisdiction, 0 otherwise; −2 log likelihood ratio = 149.28; pseudo $R^2 = .47$.
*$p < .05$.
**$p < .01$.
***$p < .001$.

tection under the Court's doctrines. To stake a claim for the redeeming social value of one's film or magazine invites the Court to judge for itself. Participants are well aware of the scant social and literary value of most of these materials. Thus a claim of literary or social significance suggests to the Court that the material may not be obscene as a matter of law, or that the petitioner does not conceive of them as obscene and has made a bet on this belief, or both.

The ideological and partisan balance of the Court makes a difference in both decisions on the merits and on composition of the agenda (Caldeira and Wright 1988, 1990; George and Epstein 1992; Segal 1984, 1985). Turnover in membership should translate into shifts in judicial policy and in the composition of the agenda. During the second half of the period of our study, the Supreme Court showed much less sympathy to libertarian claims in litigation on obscenity (e.g., Hagle 1991; McGuire 1990) and gave the issue lower priority on the plenary agenda (see Figure 1).[5] We would expect the successors to the Warren Court to rank litigation on obscenity as a lower priority. And we would expect the Warren and Burger courts to have responded somewhat differently to the various considerations in decisions on the composition of the agenda. Many would point to the doctrinal shift in *Miller* v. *California* as demarcating the differences in the Court's approach to obscenity. Our data, however, reveal that the justices' treatment of petitions in this area began to shift several years earlier, just after Warren Burger was elevated to the chief justiceship.[6] The collective effects of the four Nixon appointees produced a strong disinterest in devoting the Court's resources to the area (see Woodward and Armstrong 1979). Thus, the conservative doctrine codified by *Miller* and its progeny manifested a marked change that apparently had been in the making for some time.

THE DATA

To test our propositions, we gathered data from the set of all paid cases on obscenity filed in the Supreme

Court from 1955 through 1987 (N = 410). Libertarians (i.e., petitioners who sought to vindicate a claim based on the First Amendment and who had lost in the lower court immediately below)[7] brought 84% of these cases. We identified our cases from the summaries of filings in *United States Law Week*. To qualify for our sample, a case had to involve a government's seeking to eliminate or limit some form of allegedly lascivious expression. These forms of expression most commonly include magazines, books, newspapers, pictures, and films. This rule excluded cases in which offensive speech or restrictions based on time, place, or manner figure as the main issues.[8] The cases we have chosen dealt squarely with the question of obscenity, unadorned by related issues.

The data come from the original petitions and responses, briefs amicus curiae, opinions, and records filed at the Supreme Court Law Library and from microformed and microfilmed versions of those same materials in the Bureau of National Affairs' *Briefs and Records of the Supreme Court of the United States*. For our dependent variable, we have chosen the Court's decision to grant or deny certiorari or to note probable jurisdiction or not on a writ of appeal from a decision in a lower court. It is coded as 1 where the justices granted certiorari or noted probable jurisdiction, and 0 otherwise. The justices granted review in 23% of these cases (95 grants, 315 denials)—well above the normal rate, in part because we have so many appeals. We operationalize professional obscenity litigator as any lawyer who represented a libertarian client in two or more cases.[9] These lawyers took part in 189 of the 410 cases; amici curiae, in 24.

THE RESULTS

Simple bivariate analyses of decisions on plenary review and the participation of professional obscenity lawyers and amici curiae indicate strong preliminary support for our chief hypotheses. If a brief amicus curiae supported a libertarian petitioner, the Court granted review in 71% of the cases, in contrast to 20% in the entire sample. Libertarian amici fared significantly better than did proscriptionists. Thus, if a libertarian amicus filed in support of certiorari or jurisdiction, the Court granted plenary jurisdiction in 88% of the cases; for a proscriptionist amicus, only 38%. The result for a professional obscenity litigator is not quite so sharp; but in the presence of one of these experienced lawyers, the Court granted certiorari or noted probable jurisdiction in 26% of the cases. If neither a professional obscenity lawyer nor an amicus curiae were present, the Court bound over only 18% for full treatment. Bivariate results for the Warren and Burger courts illustrate significant changes in the positions of litigators and amici. If an amicus curiae filed a brief or a professional obscenity litigator participated, the Warren Court granted plenary review in 75% and 60%, respectively, of the cases; the Burger Court, 67% and 13%.

Do these results persist in the midst of multivariate controls? We specify the probability model of the Court's granting certiorari or noting probable jurisdiction of an appeal on the *i*th case on the docket as follows:

x_{i1} = 1 if a professional obscenity litigator (a lawyer who had filed two or more petitions on obscenity cases in the Supreme Court) appeared for the petitioner, 0 otherwise
x_{i2} = 1 if one or more amicus briefs were filed in support of the petitioner, 0 otherwise
x_{i3} = 1 if the United States was the petitioner, 0 otherwise
x_{i4} = 1 if a state was the petitioner, 0 otherwise
x_{i5} = 1 if a local government was the petitioner, 0 otherwise
x_{i6} = 1 if a bookstore was the petitioner, 0 otherwise
x_{i7} = 1 if a theater was the petitioner, 0 otherwise
x_{i8} = 1 if an individual was the petitioner, 0 otherwise
x_{i9} = 1 if a corporation was the petitioner, 0 otherwise
x_{i10} = 0 if no discernible regulation was mentioned in the brief or if the government prevailed in the court below—otherwise, 1 for prosecutions, 2 for injunctions that were not prior restraints, 3 for seizures, 4 for licenses, and 5 for injunctions that were prior restraints[10]
x_{i11} = 1 if the libertarian petitioner alleged a violation of the First Amendment, 0 otherwise
x_{i12} = 1 if the libertarian petitioner claimed that the allegedly obscene materials were of literary value or not obscene as a matter of law, 0 otherwise
x_{i13} = 1 if the case was an appeal, 0 otherwise
x_{i14} = 1 if the petitioner alleged a conflict between two or more lower courts or between the Supreme Court and a lower court, 0 otherwise
x_{i15} = 1 if the court immediately below had reversed the trial court or equivalent, 0 otherwise
x_{i16} = 1 if the case was disposed of by the Burger Court; 0, if by the Warren Court.[11]

Because it is quite plausible to anticipate important differences in the calculi of the Warren and Burger Courts in granting and denying plenary review, we also specify a series of multiplicative terms to pick up changes in the slopes for the other independent variables in the model. We estimated the coefficients in the model through a maximum likelihood probit procedure. The estimated coefficients and the associated t-statistics appear in Table 1.[12]

Our statistical results indicate strong support for the primary hypotheses. The presence of a professional obscenity litigator and an amicus curiae in support of a libertarian petitioner both increased the likelihood of review. These coefficients, as we shall see shortly, carry substantial weight and are statistically secure. The size and strength of these coefficients are all the more impressive in light of the large number of variables in the statistical model. If, for example, we constrain the value of all of the independent variables to their means and assume that no professional obscenity lawyer were present, the probability of plenary review is .12. Then, if we assume that a petition were brought by expert counsel, the probability of plenary review increases to .24. More impressively, this same exercise shows a increase from .15 to .70 for the presence of one or more briefs amicus curiae throughout the Warren and Burger Courts. If a professional obscenity litigator and an amicus curiae appeared in the same case, the probability of plenary jurisdiction soared to .77. Just as we had anticipated, the Court weighed the participations of amici curiae more heavily than it did professional obscenity litigators. Of course, both counted for a great deal; but for the reasons we noted earlier, the amici curiae are more credible—and therefore more potent—indicators of importance.

The results show starkly the changed situation for professional obscenity litigators under the Burger Court. We see a significant decrease in the ability of these experienced litigators to succeed in obtaining plenary review in the Supreme Court. For example, if we assume the presence of a professional obscenity litigator and constrain all of the remaining independent variables to take on their mean value, the probability of plenary review drops to .18 for the Burger Court.

Overall, the Court showed a pronounced antipathy toward three of the four varieties of libertarian litigants. The presence of a theater or individual as a petitioner actually decreased the chances of a grant; and the sign for bookstores, although marginally insignificant, indicates a similar relationship. Corporate status of a petitioner did not carry any weight one way or the other. The justices of the Burger Court did show more interest in the libertarian petitions of both individuals and theaters, but only relative to the treatment those parties received during the Warren Court. Since these litigants were so disadvantaged during the Warren era, their increased "success" under the Burger Court still did not translate into a significant number of their petitions being granted.

The paucity of cases in which the federal and local governments petitioned the Supreme Court during the Warren Court precludes the computation of coefficients for these variables, but we find no evidence of differential treatment of state governments in the sample as a whole. We do encounter strong evidence of success on the part of the United States as a petitioner during the years of the Burger Court. A positive though statistically insignificant coefficient for state governments in the Burger Court suggests a modest improvement in their fortunes.[13]

In the midst of multivariate controls, the severity of the regulation at issue made no difference in the probability of plenary review. And there is no significant difference between the responses of the Burger and the Warren courts to our indicator of the severity of regulations. Of course, in the bivariate case, both the Warren Court and the Burger Court showed a marked tendency to grant plenary review in the more extreme examples of proscriptive zeal. Our results suggest, instead, the direct impact of other variables in the model.

The results also suggest the importance of legal claims in the calculus of the Court in agenda setting. If, for example, a libertarian petitioner put forth a claim of "literary value" in defense of expression, the Supreme Court was much more likely to grant a plenary review than in the absence of such an argument. In broad terms, it bears noting that while the coefficients for the libertarian interests are negative, the parameters associated with their claims are positive. In other words, although the justices were disdainful of many liberal petitioners, the Court nonetheless took the substance of their claims quite seriously. This general tendency did not persist in the Burger Court: despite the apparent shifting fate of some of the liberal petitioners, their legal claims were viewed with considerably greater skepticism. The attraction of claims of literary value, for instance, plummeted during the Burger Court. It showed much less inclination to grant plenary review to examine whether the materials in a particular case met the often-vague standards of literary value. We see a similar, though less pronounced, decline in the impact of the First Amendment as a claim in cases on obscenity in the years of the Burger Court.

The procedural advantage of appeal over certiorari shows up in our data, but for the most part this advantage disappeared under the Burger Court. Contrary to our expectations, allegations of conflict carried a negative but marginally insignificant coefficient: the more allegations of conflict, the lower the probability of plenary review. Few, if any, of the cases in our sample contained clear conflicts between lower courts, so this result may reflect the lack of credibility of such claims. In contrast to the justices of the Warren Court, the Burger Court responded in a moderately positive fashion to allegations of conflict. Last, reversals between the lower courts did not incline the Supreme Court toward plenary review, and there is actually a negative coefficient. For reversal in the lower courts, as for allegations of conflict, we see something of a change in the years of Chief Justice Burger. The coefficient for reversals is positive for the Burger Court, but when it is discounted by the Warren Court's negative approach, the Burger Court appears to have been largely indifferent to reversals.

In general, the Warren and Burger Courts brought unique perspectives to case selection in obscenity, placing different weights on several of the criteria in our model. Different judges and different weights resulted in few cases on the plenary agenda. But as the coefficient for the Burger Court indicates, we find only modest evidence of a linear effect; the Burger Court did not simply lower the rate at which it reviewed cases in obscenity.

Overall, the statistical support for our primary hypotheses strikes us as strong and impressive. Of course, not everyone accepts at face value the causal nature of the relationship between the participation of lawyers and interest groups and the Court's decision on plenary review. Critics have accused litigators and organized interests of choosing the "sure winners" among the various available cases in order to build up an impressive record of victories. For example, an organized interest might scan a case for major indicators of "importance" and then file a brief if its leadership believed the Court would grant certiorari. Similarly, a lawyer who specialized in litigation on obscenity might push forward only those cases he or she saw as clearly eligible for review. Lawyers and interest groups, the argument goes, would simply simulate the decisional calculus of the Supreme Court, so we could not impute influence to either one.

We find no support for this view. In the law of obscenity, amici curiae responded to quite different considerations in comparison to professional obscenity litigators. From the Warren to the Burger Courts, the amici curiae, like the specialized practitioners, exhibited sharp departures in the set of criteria to which they responded. Importantly, professional obscenity litigators and amici curiae used criteria to choose cases quite different from those of the Supreme Court. Our additional analyses of these data strongly suggest the independence of the decision-making processes of lawyers, amici curiae, and the Supreme Court.[14]

CONCLUSION

For our set of terms in the Supreme Court, lawyers make a significant difference in the shape of the agenda in the law of obscenity. Libertarian petitioners represented by a professional obscenity litigator enjoyed an important advantage in the stage of case selection. Organized interests did not appear as often as amici in support of petitioners, but when they did, their presence carried considerable weight in the decisional calculus of the Supreme Court. Indeed, the presence of an amicus curiae raised the probability of plenary review even more than did the appearance of a professional obscenity lawyer. Under the regime of the Burger Court, the influence of professional obscenity litigators declined sharply, but organized interests as amici curiae continued to make an important contribution in the selection of cases on obscenity.[15]

Our research raises a number of issues for future research. Is the role of lawyers and organized interests similar to the pattern we observed here in other areas of the law? We can envision some areas in which repeat lawyers and organized interests would participate less often and hold less sway than in the law of obscenity. For example, organized interests participate less often as amici curiae prior to plenary review in the criminal law. We adopted a simple measure of professional litigator. The impact of professional litigators might emerge even more clearly if scholars create refined measures of this concept. We offered an account of the clout of professional obscenity litigators and organized interests but would like to pin down the bases of influence in agenda building more precisely. We still know very little about the strategies of organized interests and professional litigators in the selection of cases for filing appeals even though our data shed some light on these calculations. We would do well in the future to focus directly on these crucial actors in the formation of the Supreme Court's agenda.

Recent developments in the Supreme Court underscore the importance of our results. In the last several years, the Court has placed fewer and fewer cases on the plenary agenda. Some commentators and practitioners attribute this decline to the elimination of the writ of appeal in most cases and to the decrease in the number of intercircuit conflicts as Reagan and Bush appointees have come to dominate the intermediate appellate courts. Year in and year out, conflicts and appeals have occupied a substantial portion of the Court's plenary agenda. Now, as a result of the decline of these categories, the Supreme Court's task of identifying important cases has become even more difficult than in the past (see Greenhouse 1992, 7). Without traditional indicators such as the writ of appeal or intercircuit conflicts, the Supreme Court in composing the plenary agenda may well rely even more heavily on experienced lawyers and amici curiae to differentiate the important from the unimportant cases.

Notes

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1. For our account of the organizational politics of obscenity, we rely heavily on Kobylka 1987 and 1991, now the authoritative works on this subject, and on our own research in the *Records and Briefs of the Supreme Court of the United States*.

2. Of course, organized interests perform an important, sometimes crucial, role on the merits (in the Supreme Court and elsewhere) as amici, direct representatives, consultants, and the like (see Epstein 1990; Epstein and Rowland 1991).

3. Our categories do not exhaust the kinds of petitioners. A number of cases involve arcades, newsstands, and the like.

4. Many of the libertarian petitioners concentrate on other legal questions, including due process of law, *scienter*, and prior adversary hearings.

5. Justice Stevens noted the change in the mid-1970s: "There is no reason to believe that the majority of the Court which decided *Miller v. California* . . . is any less adamant than the minority. Accordingly, regardless of how I might vote on the merits after full argument, it would be pointless to grant certiorari in case after case of this character only to have *Miller* reaffirmed time and time again" (*Liles* v. *Oregon* 1976).

6. Examination of individual cases points to *Miller* as a lagging indicator of a shift in the Burger Court's priorities.

7. On occasion, a libertarian claimant who had won below sought certiorari or probable jurisdiction to vindicate legal arguments on which the respondent had prevailed in the court immediately below or filed a cross-petition in response to an opponent's petition for certiorari.

8. Other scholars have included either or both of these kinds of cases for understandable reasons (e.g., Hagle 1991; Kobylka 1987); but we think our approach isolates the perennial issues of obscenity from more general questions of criminal procedure, regulatory policy, and other varieties of offensive speech (see Stone et al. 1986, 1114–45). Accordingly,

we have discarded some of the better-known cases often considered under the rubric of obscenity. For instance, we exclude *Cohen* v. *California* (1971), *Erznoznick* v. *Jacksonville* (1971), *Schad* v. *Borough of Mount Ephraim* (1981), and *Young* v. *American Mini Theatres* (1976).

9. This is, we admit, a fairly liberal definition. Remember, however, that we are employing cross-sectional data and have no way of knowing a priori whether a single appearance by a lawyer is his or her sole appearance or representative of a pattern of participation. We can make credible assumptions about some attorneys (e.g., the solicitor general) regardless of the number of appearances he might make across a given time period. We do not suspect that a second case heaps additional benefit on a lawyer and advantages him over counsel with only one case. Rather, we are simply assuming, absent other indicators of differences between counsel, that a lawyer with multiple cases is probably different along some qualitative dimension from a lawyer with only a single case. For more on this point, see McGuire 1993.

10. We established this ordering based on our reading of the cases and commentary and our sense of the severity of the threat to expression each poses. Then we tested our notions against the data. We used a set of dummy variables for each of these conditions. The size of the coefficients associated with the dummies confirms our ordering.

11. The reader might note that our data extend into the early tenure of the Rehnquist Court. This involves only a handful of cases and changes none of the results.

12. We have considered a wide range of other independent variables. None of those excluded in our final model proved significant in preliminary analyses. For example, while Hagle (1991) demonstrates the important effect of region on outcomes on the merits in cases on obscenity, we found no evidence of regional influences in our data. Our probit analyses yielded no evidence of any influence, one way or the other, of "fact patterns" or other legal arguments in the decision on plenary review in either the Burger or Warren Courts.

13. Epstein and O'Connor (1988) establish that state governments with expertise in litigating before the Court tend to enjoy more favorable treatment from the justices (see also George and Epstein 1992). Therefore we thought it important to differentiate between repeat players and one-shotters. Unfortunately, there are too few cases to distinguish the effects of different states during the Warren Court from their effects under the Burger Court; thus although there are good theoretical reasons for estimating such an equation, we cannot do so. We were, however, able to test for the influence of these two different types of state governmental petitioners in general and found no evidence that repeat players were any more advantaged in case selection than were one-shotters.

14. In particular, we ran a two-stage probit analysis in which we created an instrument in the first stage for professional obscenity litigators. There is no evidence of simultaneity in these results.

15. Of course, our approach examines but one slice of the Court's agenda building. The process of case selection is not isolated by issue area; petitions for review in the law of obscenity compete not only with one another but with pleadings from myriad other issues, as well.

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THE RELATIVE-GAINS PROBLEM FOR INTERNATIONAL COOPERATION

Modern realism claims that the fear that others will enjoy relatively greater benefits frequently impedes international cooperation. Recent articles in this Review *by Duncan Snidal and Robert Powell modeled conditions under which the impact of relative gains varied. Joseph Grieco criticizes Snidal's model as based on assumptions that allow him to avoid, rather than confront, the realist arguments. He also argues that Powell's model, while constructive, ignores important additional sources of sensitivity to relative gains. In response, Powell discusses the value of alternative assumptions about preferences and constraints in international relations. Snidal defends his analysis and presents an additional proof to support the independence of his central result—the diminishing impact of relative gains with increasing numbers of states—from assumptions of concern to Grieco. Both responders emphasize their work as contributing to a contextually rich theory of international politics that builds on elements of both realism and neo-liberalism.*

COMMENT

Two contemporary approaches to international relations—modern realism and neoliberal institutionalism—compete in seeking to understand how the anarchical context of the international system inhibits joint action among states that otherwise share common interests and how states sometimes overcome those inhibitions and achieve cooperation.[1] Neoliberals argue that in the context of international anarchy, states that face mixed interests often fail to cooperate because they are tempted to cheat and fear being cheated. They argue further that this problem of cheating can be resolved if states create international institutions that help them to work with one another on the basis of tit-for-tat strategies of conditional cooperation.

Scholars in the realist tradition have long recognized that cooperation is an important feature of world politics. They have, for example, studied the security dilemma and its effects on cooperation (Jervis 1978; Van Evera 1985); hegemonic leadership and economic cooperation (Gilpin 1971, 1975, 1987; Krasner 1976, 1985, 1991); linkages between military alliances and economic cooperation (Gowa 1989); the implications of systemic change for cooperation in post–Cold War Europe (Mearsheimer 1990); and power balancing and military alliances (Aron 1973, 118–19; Morgenthau 1948, 138–39; Snyder 1990; Walt 1987, 1988; Waltz 1979, 125–27). While emphasizing that international cooperation is possible, realists have argued that it is harder to achieve and more difficult to maintain than the institutionalist tradition suggests. They have suggested that anarchy impedes cooperation not only because it generates cheating problems but also because it causes states to worry that partners might achieve relatively greater gains from collaboration and, thus strengthened, become more domineering friends in the present or possibly more formidable foes in the future. Hence, realists have argued that states must overcome two barriers to cooperation: the problem of enforcement and the problem of relative gains. They have also stressed that by ignoring the latter, neoliberalism provides an incomplete understanding of the problem of cooperation.

Two essays focusing on the relative-gains aspect of the realism–neoliberalism debate have recently appeared in the *Review*. In the first, Duncan Snidal (1991a) claims to have developed a model that uses realist assumptions but discredits realist arguments about the relative-gains problem for cooperation. Yet there are at least three serious defects with Snidal's analysis: (1) Snidal bases his model on assumptions that allow him to avoid rather than confront realist arguments about relative gains and cooperation; (2) many of Snidal's key ideas simply echo realist propositions; and (3) when Snidal discusses cooperation in the real world, he largely abandons his own model and relies on realist arguments.

A more helpful discussion of the relative-gains problem for cooperation is provided in the second essay, by Robert Powell (1991). He provides a model that confirms the basic realist argument that states in anarchy may have security-based concerns about the distribution of gains from cooperation. At the same time, aspects of Powell's model lead to expectations as to the prevalence of state concerns about relative-gains that differ from those suggested by realist theory. These divergences in expectations highlight important questions for future research. Hence, by both confirming and challenging realist arguments, Powell's essay provides the basis for productive discourse in the field of international relations.

SNIDAL'S "CHALLENGE" TO REALIST THEORY

Duncan Snidal suggests that according to realists, "states are seen to seek relative gains and the inference is drawn that this inhibits cooperation" (1991a, 703).[2] He notes, "I do not address the plausibility of relative gains seeking as a description of international behavior, I stipulate it as an assumption." He does this in order to refute realist arguments about the problem of cooperation: "By accepting the assumption of relative gains maximization, I show that it does not have the general inhibiting effect on international cooperation widely ascribed to it" (ibid.).

Using what he takes to be the realist assumption about state preferences to "seek" relative gains, Snidal presents models of two-state and large-*n* interactions. He claims that the former provides partial support for his version of realist thinking about relative gains and cooperation.[3] On the basis of the latter, however, which is obviously more relevant for world politics, Snidal argues that relative-gains calculations do not inhibit cooperation to the degree that realism suggests. Thus, "the relative gains argument cannot provide a decisive response to the institutionalist claim that decentralized cooperation is possible under anarchy"; indeed, "relative gains can save the realist argument only in the two-actor world and perhaps demonstrate that institutionalists underestimate the difficulty of cooperation in other very-small-*n* cases. But these are much weaker claims than realists have made in uncritically transferring relative-gains arguments from the two-actor world to international politics more generally" (1991a, 719).

Snidal seems to present a powerful challenge to realist thinking about the question of relative gains and international cooperation. Yet upon closer scrutiny, it becomes clear that his arguments fail to engage, let alone refute, the realist position. There are three major flaws in his analysis.

Snidal Solves the Relative-Gains Problem by Assumption. Snidal assumes that the gains from cooperation are distributed in such a way that there is and can be, by definition, no relative-gains problem for either partner. In addition, he assumes that if cooperation somehow generates gaps in gains, these gaps (again by definition) are narrowed to the satisfaction of the disadvantaged partner.

Snidal's large-*n* model is based on the assumption that if any two states cooperate, each will enjoy "constant returns." This means, according to Snidal, that the "gains from cooperation are proportional to the size of the involved states *and are shared equally between them*" (1991a, 715, emphasis mine). As a result, "in terms of *relative position* (as measured by absolute gaps between states), cooperation does not lead to relative gains" (ibid., emphasis mine). He goes on to specify that for the two referent states in his large-*n* model, state *i* and state *k*, "Since *i* and *k* gain equally from cooperation, *neither achieves any relative gain over the other through joint cooperation*" (p. 717, emphasis mine).

Snidal emphasizes that this constant-returns assumption is "key" to his model and that it "is most important in the general argument" (1991a, 715). What happens if we relax this "key" and "most important" assumption? Snidal provides the answer in a note: "Unequal gains diminish the incentives of one side to cooperate (and increase the incentives of the other side) for both absolute *and relative gains* reasons" (p. 725, n. 29, emphasis mine). Hence, if we assume that states receive unequal gains, we find that this can create a relative-gains problem. The opposite is of course also true: if states *by assumption* receive payoffs that do not affect their relative position, then by definition the whole question of gaps in gains from cooperation is simply irrelevant to them. Given its constant-returns assumption, Snidal's large-*n* model can only yield the "finding" that gaps in mutual gains do not affect the propensity of states to cooperate.

Moreover, while the constant-returns assumption is a "key" to his model, it is also his Achilles' heel for any engagement with realist theory. The main realist statement about the relative-gains problem, presented by Kenneth Waltz (which Snidal himself cites as the basis for his understanding of the realist perspective on this matter) makes it clear that for realists, the emergence of the problem is predicated upon the prospect of gains that are *unequal* and lead to a *change in relative position* among partners. That is, "When faced with the possibility of cooperating for mutual gain, states that feel insecure must ask how the gain will be divided. They are compelled to ask not 'Will both of us gain?' but 'Who will gain more?' If an expected gain is to be divided, say, in the ratio of two to one, one state may use its *disproportionate gain* to implement a policy to damage or destroy the other" (1979, 105, emphasis mine; cited at Snidal 1991a, 703). Waltz goes on to argue that "even the prospect of large absolute gains for both parties does not elicit their cooperation so long as each fears how the other will use its *increased capabilities*" (ibid.).

Snidal, then, has excluded *by definition* the exact situation that realists posit as triggering the relative-gains problem for cooperation, namely, that one partner does better than another and enjoys an advance in relative position over the latter. Indeed, the situation analyzed in his model—one in which states receive payoffs in such a way that there is no change in relative position—is exactly the one in which realists would not expect the relative-gains problem to be in operation. Grounded as it is on the constant-returns assumption, Snidal's large-*n* model simply avoids the realist argument about the relative-gains problem for cooperation.

Snidal avoids the relative-gains problem in a second way. He assumes that gaps in gains, if they do emerge, will be automatically adjusted. Snidal acknowledges that relative-gains problems may limit "the range of viable cooperative arrangements because states will not accept deals that provide disproportionately greater benefits to others." However, he asserts that this is a "less important" problem because "if distribution is the primary relative gains problem, states can alter the terms of the cooperative arrangement or offer side payments until the distribution of gains is sufficiently proportionate" (1991a, 703).[4] Thus relative-gains problems might arise, but if they do, reforms or side payments *by definition* resolve them.

States do recraft agreements and provide side payments in order to address relative-gains concerns of disadvantaged states, and international institutions can promote cooperation precisely because they often help facilitate these compensatory arrangements (see Grieco 1990, 222–26, 230–31, 233–34). The problem with Snidal's treatment of this important matter is that he fails to offer any basis for the assertion either that these *potential* solutions to relative-gains problems are typically undertaken or, if they are, that they are able to ameliorate such problems reliably. In fact, the available scholarship suggests that effective reforms and side payments are sometimes provided but sometimes are not, and when satisfactory adjustments are not made, the relative-gains problem can hamper or cripple cooperative arrangements (ibid., 47–48, 220–26, 230–31; Krasner 1991; Mastanduno 1991; Tucker 1991).

Moreover, saying that relative-gains problems do not inhibit cooperation because states *can* ameliorate them through reforms or side payments is equivalent to saying that cheating problems do not inhibit cooperation because states *can* resolve them by establishing verification and sanctioning arrangements. Both assertions are of course true *in principle*. Yet international relations is an important field of study in large measure because we know that effective solutions to cheating and relative-gains problems are sometimes available and sometimes are not, and we want to know why. For Snidal, relative-gains issues are either defined as nonexistent by assumption or are defined from the outset as easily settled and therefore uninteresting. In the real world, however, they are frequently problematic and sometimes vitally important.

In short, Snidal presents us with the following line of analysis: *by assumption*, states receive equal gains from cooperation or readily redress gaps if they arise; therefore, states do not suffer losses in relative position if they cooperate; by consequence, states are not inhibited from cooperating because of concerns about gaps in mutual gains; thus, realism is wrong to emphasize the relative-gains problem for cooperation. Yet in the real world, states sometimes do believe they might receive or are receiving unequal gains leading to an unfavorable change in relative position, and such concerns can sometimes impair cooperation. Moreover, Snidal's constant-returns assumption and his premise that relative gains are resolved as a matter of course cause his model to fail to engage, let alone challenge, realist theory.

Snidal Echoes Realist Arguments. Realists would not consider Snidal's main point—that the existence of third parties reduces relative-gains concerns between any two partners—to be controversial. In *Cooperation among Nations*, for example, I suggest that "from a realist viewpoint, the coefficient for a state's sensitivity to gaps in payoffs—k—can be expected to vary but always to be greater than zero" (Grieco 1990, 45). The sentence emphasizes that states are typically concerned in some measure if gaps in gains favor partners but also that the *severity* of such concerns may vary. I then identify several factors that may affect the intensity of k, one involving third parties: "To the degree that there is a common enemy posing a clear and present danger, a state may actually welcome increases in an ally's capabilities" (p. 46).[5] Similarly, Robert Jervis, citing Franco–British cooperation to counter Nazi Germany, observes that "when a state believes that another not only is unlikely to be an adversary, but has sufficient interests in common with it to be an ally, then it will actually welcome an increase in the other's power" (1978, 175). Hence, Snidal's model reiterates a realist idea: if two partners face a challenger, their bilateral relative-gains concerns go down, and this makes cooperation between them easier.

Moreover, realist theory has already suggested that changes in the number of states to which a state compares itself can affect the severity of the relative-gains problem for cooperation. For example, I have argued that neoliberals, believing that cheating is easier to prevent if there are fewer partners, would expect that "states with a choice would tend to prefer a smaller number of partners" (Grieco 1990, 228). I then offer a competing realist hypothesis: "If it is uncertain about which partners would do relatively better, the state will prefer more partners, for larger numbers would enhance the likelihood that relative gains advantaging (what turn out to be) better-positioned partners could be offset by more favorable sharings arising from interactions with (as matters develop) weaker partners" (ibid.) My argument is that as the number of states in an agreement increases, relative gains concerns between any two partners should go down. Snidal's argument is that as the number of states not in an agreement increases, relative-gains concerns between any two partners should go down. We are looking at different third parties (I at third parties in an arrangement, he at third parties outside it); but we are both saying that increases in such third parties can reduce the relative-gains concerns of a state in regard to a partner. Again, Snidal is not saying much that is new.

Snidal's core intuition about third parties and the relative-gains problem runs parallel to realist thinking. Given this area of agreement, it would be useful to have a formal model of the effect of third parties on the relative-gains calculations of partners. Unfortunately, Snidal's large-n model is unable to shed light on this subject, for, as I noted, the assumption of constant returns means that by definition there is no relative-gains problem for cooperation in that model. Of course, Snidal and realists may disagree about the *extent* to which relative-gains concerns are likely to decrease with increases in third parties. Realists would expect partners typically to be somewhat worried about gaps in gains regardless of the number of parties in or outside an arrangement. Snidal apparently does not think states have such concerns to a significant degree as long as there is even a moderate number of third parties—not, that is, until he begins to discuss cooperation in the real world.

Snidal Abandons His Own Model and Relies on Realist Arguments. In a discussion of "stylized facts" about international cooperation, Snidal seeks among other things to assess the effects of differences in the size of states on their propensity to experience relative-gains concerns. When he does so, he abandons his own arguments and confirms realist arguments that states may experience relative-gains concerns even if there are third parties.

First, Snidal acknowledges that *small states* may have concerns about gaps in mutual gains as they interact with larger states. He says, "Although I did not assume it earlier, it is reasonable to believe that states are warier of larger states with regard to relative gains. Thus, for any pair of states, the smaller state will be more concerned with the relative gains consequences of their interaction (i.e., $w(ij) > w(ij)$ [*sic*] if $s(i) < s(j)$, ceteris paribus)" (1991a, 720). In making this observation, Snidal is acknowledging that the relative-gains problem, as it is articulated by realists, may come into operation even in a large-*n* situation (remember the ceteris paribus clause). In doing so, he is abandoning the main argument of his essay and confirming realist expectations.

Second, Snidal acknowledges that *large states* may experience concerns about gaps in gains from cooperation. After he notes that smaller states may have relative-gains concerns in regard to a larger partner, he suggests that the larger state may allow the smaller states to achieve disproportionate gains in order to ameliorate their relative-gains concerns. The smaller states may thereby catch up with and become less concerned about the hegemon. However, Snidal then says that "declining relative predominance increases the hegemonic state's concern for relative gains with other states, especially any rising challengers. The net result is increasing pressure from the largest actor to change the prevailing system to gain a greater share of cooperative benefits."[6] Hence, according to Snidal, "whereas in the 1950s and 1960s Europe, Canada, and the Third World complained of U.S. dominance in trade and investment, in the 1970s and 1980s it was the United States that worried about foreign—especially Japanese—relative advantage." Again, "The United States now actively seeks to alter international trading rules in its favor whereas in the early postwar period it accepted special exceptions for European and less-developed states" (1991a, 720).

This discussion by Snidal of U.S. behavior conflicts with the core analytical claim he makes in the same paper on the basis of his large-*n* model. The claim is that as the number of third parties goes up, the relative-gains concerns of any particular state go down. Of course, whether we are talking about the 1950s or the 1980s, we are talking about *very*-large-*n* situations. Hence, if the model were right, we would see few if any instances of the relative-gains problem in postwar history; the fact that we see them frequently (Snidal himself cites the example of the United States) casts serious doubt on his model. Also, there were more independent states in the 1980s than in the 1950s. Yet despite this trend—which according to Snidal's model should have caused America to be progressively less sensitive to gaps in gains enjoyed by its partners—Snidal himself suggests that the United States became not less but more sensitive about such gaps.

Perhaps U.S. relative-gains concerns were affected during the postwar period not by the total number of nations in the international system but by the number of important states. Yet there is nothing in Snidal's specification or discussion of his large-*n* model that would support this qualification. Even if it were true that the United States focused only on major states during the postwar period, the number of such states also increased during that period (especially in the international economy, the domain to which Snidal refers in his essay), again suggesting that the United States should have become less sensitive to gaps in gains, not more sensitive. Regardless, then, whether one refers to the total number of states in the 1950s and the 1980s (they were both large-*n* periods), to the change in the total number (there was a big increase), or to the change in the number of major actors (that also went up), the "stylized fact" that the United States became more sensitive in the 1980s compared to the 1950s about gaps in mutual gains contradicts the core claim that Snidal makes on the basis of his model.

In addition, Snidal argues that America's partners grew in power during the postwar period not because they cheated the United States but because they were faithful to agreements in which all enjoyed absolute benefits but in which the partners, with U.S. consent, achieved disproportionate gains. Yet, as noted earlier, unequal gains from cooperation are explicitly excluded in the presentation of his large-*n* model. Here Snidal also suggests that a partner can become increasingly concerned about such gaps; yet again, this possibility was assumed away in the main section of his paper. And what Snidal recognizes at the end of his essay to be an important feature of the contemporary world scene (i.e., an America, increasingly worried about gaps in gains from cooperation and acting to improve its share of those gains) is exactly what at the beginning (703) he had characterized as "the less important" of ways that relative-gains issues might impede international cooperation. Snidal's willingness to abandon his own large-*n* model as soon as he tries to discuss real-world events underscores the limitations of that model as a basis for analysis of international affairs.

Third, and finally, Snidal's discussion of the relationship between state size and the degree of state sensitivity to gaps in gains—measured by what Snidal calls "relative gains weights"—refutes the claims he makes against realism and, again, supports realist arguments. In a note to the statement just cited regarding declining hegemons and their increasing sensitivity to gaps in mutual gains, Snidal says that "the relation between size and relative gains weights may be more complicated than the monotonic relation used here. A plausible curvilinear specification is that relative gains concerns peak when states are roughly equal and drop off when one state is either far behind, or far ahead of, the other" (1991a, 725, n. 33).

There are at least three points to be made about this passage. First, here again, Snidal is compelled to acknowledge that gains from mutual cooperation can be unequal and that states may be sensitive to such gaps (hence the sensitivity weights). Second, Snidal's discussion of "relative gains weights" in regard to differences in *mutual* gains from cooperation suggests that we ought to build them into our models; yet Snidal fails to do so in his large-*n* model and instead avoids what he acknowledges is an important matter by assuming that there are equal gains from mutual cooperation. Finally, the factor that Snidal stresses in his large-*n* model—the number of affected third parties—drops out of sight and seems to have no effect on these weights.

In sum, Snidal's large-*n* model is based on assumptions that solve the problem of relative gains by definition and allow him to avoid rather than confront realist arguments about the relative-gains problem. On the most important issues, Snidal simply echoes realist propositions. Finally, when he attempts even a cursory discussion of real-world cooperation, he is driven away from his major claims and back toward the realist position. Hobbled by these and other failings, Snidal's analysis hardly challenges realist theory. Indeed, because his analysis solves the relative-gains problem by assumption, it is difficult to see how it advances analysis of this problem for international cooperation. Yet when we turn to the essay by Robert Powell, we find that there are good reasons to remain optimistic about the prospects for progress in our understanding of this most important question.

POWELL'S MODEL AND PROGRESS IN INTERNATIONAL THEORY

Robert Powell presents in his 1991 essay a formal model showing that in the context of international anarchy, gaps in otherwise mutually positive gains from joint action will detract from a state's utility (and perhaps cause it to choose not to cooperate) *if* it fears that the advantaged partner might employ the increase in capabilities produced by the gap in gains in such a way as to be a greater military threat. This general finding supports the basic realist argument about the relative-gains problem for cooperation. However, there are differences between Powell's specification of, and realist arguments about, the range of causes of state concerns about relative gains. This is an important matter, for the range of sources of state concerns about gaps in gains is likely to be a key determinant of the incidence of the relative-gains problem for cooperation.

As noted, states in Powell's model are concerned about relative gains if they fear that they might fight a war with their partners. Powell emphasizes that to formulate a model of realism's understanding of state concerns about relative gains from cooperation, it is vitally important to include this "specter of war" as a possible development, and in this regard he criticizes the use by neoliberal institutionalists of repeated games because the latter to date do not depict the possibility that force may be used by the actors in such games (1991, 1309–11). He stresses that he has sought to remedy this problem: "I develop a very simple and highly stylized formalization of the problem of absolute and relative gains. States will now have the explicit option of using force if that seems to them to be in their best interest. More importantly, the model has been explicitly designed so that relative gains and losses matter because they affect a state's ability to use force successfully to further its interests" (p. 1311). Having included the possibility of force in his model, Powell determines that "if the use of force is at issue because the cost of fighting is sufficiently low, cooperation collapses in the model. This is in keeping with the expectations of structural realism." On the other hand, "if the use of force is no longer at issue, cooperation again becomes feasible. This is more in accord with neoliberal institutionalism" (ibid.; see also p. 1314).

Realists would agree with Powell that states fearing that a partner might use force against them will be concerned about gaps in mutual gains advantaging that partner. Yet realists would argue that there are at least two additional sources of state concerns about relative gains: uncertainties about one's partners and the efficacy of force, and fears about the nonmilitary consequences of gaps in gains. They would also argue that this wider range of sources of state concerns about gaps in gains causes the relative-gains problem to come into operation more frequently than might be expected on the basis of Powell's model.

Realists would argue that uncertainty about one's partners may induce relative-gains concerns in states; indeed, I have suggested that "realists find that defensive state positionalism and the relative gains problem for cooperation essentially reflect the persistence of uncertainty in international relations" (Grieco 1990, 45). Realists emphasize that in the context of international anarchy, states cannot be certain how states that are friendly in the present will choose to act in the future. As Robert Jervis suggests, "Minds can be changed, new leaders can come to power, values can shift, new opportunities and dangers can arise" (1978, 168). This uncertainty about one's partners, according to realists, is a vitally important cause of state concerns about relative gains. Waltz, as noted, says that states worry that an advantaged partner may become a threat in the future. He goes on to emphasize that "the impediments to collaboration may not lie in the character and the immediate intention of either party. Instead, the condition of insecurity—*at the least, the uncertainty of each about the other's future intentions and actions*—works against their cooperation" (1979, 105; emphasis mine). Hence, even if a state believes that a partner will not use gaps in mutual gains against it in the *foreseeable* future, it may still be wary about such gaps, because it cannot be certain that new leaders or a new regime might not yield a partner more willing and motivated to so employ gaps in a manner harmful to it.

In addition, states may experience uncertainties about the efficacy of force. In Powell's model, the "technology of warfare" determines the "cost of fighting," and the latter drives the possibility that force might be used between two partners and therefore drives their concerns about relative gains (1991, 1312, 1314). If the "cost of fighting" goes up, the efficacy of force goes down; therefore, state concerns about relative gains also go down. Powell makes no explicit assumption about the capacity of states to assess at a given point in time either the "technology of warfare" or the resulting "cost of fighting." However, it would seem to be a fair inference that within the framework of his model, states would have to have a capacity to make such assessments; for only if they did so could they estimate whether force is or is not "at issue" and thereby judge whether they should care about relative gains. Yet realists would suggest that states may not know at a particular time if the costs of war are such that using force is or is not irrational; that if they believe that war is irrational, they cannot be sure that their partners share this cost–benefit analysis of the efficacy of force; that even if they believe that they and their partners all hold a negative assessment of the efficacy of force in the present, they cannot be certain that the "objectively" true cost–benefit ratio will remain biased against the use of force in the future; and, finally, that if states believe that force will remain irrational in the future, they cannot be sure that their partners will continue to share the same view about this matter at that future time. Moreover, realists would argue that in light of the possible consequences of making the wrong assessment about the future efficacy of force (at the extreme, the price of being wrong might be enslavement or even extinction), states are likely to approach these matters as extremely risk-averse actors. This combination of high uncertainty about the efficacy of force and low tolerance for risk about that subject is likely to cause states to worry about gaps in gains advantaging partners to some degree even if they believe that at present the use of force between them is not at issue.

Hence, from a realist viewpoint, two kinds of uncertainty make it difficult for states to determine whether force is or is not "at issue" with respect to particular partners: the future technology and costs of war and the future interests and goals of those partners. As a result, while accepting Powell's view that state concerns about gaps in gains are rooted in their interest in survival and security, realists would expect states, as a result of the uncertainties inherent in world politics, to have such concerns more frequently and with a wider range of partners than might be expected on the basis of his model.

In addition, realists would argue that international anarchy leads states to be concerned about gaps in gains from cooperation not just because they seek security and survival but also because they value their autonomy and independence. In a hierarchic domain (e.g., stable domestic societies), actors are relieved of the need for complete self-sufficiency, and can therefore specialize and become functionally differentiated. In contrast, anarchical environments are self-help arenas. Consequently, agents must seek to retain some minimum capacity to protect their interests. This need for self-help sufficiency leads states to value autonomy and independence: "In an anarchic realm, the units are functionally similar and tend to remain so. Like units work to maintain a measure of independence and may even strive for autarchy" (Waltz 1979, 104).

This concern for independence, Waltz argues, causes states to be wary about cooperation because they may become dependent on their partners (1979, 106–7). My view is that states may also fear that suffering gaps in gains today might result in an erosion of their independence and autonomy tomorrow. An advantaged partner, states may worry, could use its increased nonmilitary influence to force them to accept progressively less favorable terms in the arrangement in which the gap originated and in arrangements in other domains as well. Through a cumulative process of converting gaps in gains in one period into progressively better deals in later periods (but in which all would still gain in absolute terms), the advantaged partner might in time also become powerful enough to restrict (again through the application of nonmilitary forms of influence) the capacity of the disadvantaged partners for independent choice and action in the domain in which cooperation is occurring and other domains to which that domain is related.

Michael Mastanduno (1991) has demonstrated that a concern about cumulative shifts in national capabilities arising from unequal cooperative gains was a factor in U.S. policy on collaboration with Japan on the FX jet fighter and commercial space satellites. (He finds that elements of the U.S. government had similar concerns regarding high-definition television but that these concerns did not affect U.S. policy.) Mastanduno shows that the U.S. government sought in the case of the FX, and to some extent in the case of commercial space satellites, to redress distributions of gains that it viewed as disproportionately favoring Japan. He emphasizes that "the immediate concern was not military security, but economic well-being. Some U.S. officials feared that certain patterns of economic interaction with Japan, even though mutually beneficial in absolute terms, would bring relatively greater economic benefits to Japan *and over time work to the detriment of America's competitive position in the development and application of advanced technology*" (p. 109; emphasis original). Similarly, I suggest in *Cooperation among Nations* that the European Community was concerned during the 1980s that the United States would gain the most from an aggressive implementation of the Tokyo Round codes on government procurement and technical standards, that the

European Community viewed these two issue areas as having a major impact on their capacity to retain an advanced and independent technology base, and that this concern about strategically important relative gains led it to limit its commitment to the two codes (Grieco 1990, 182–209). Thus, while Powell's model would lead us to expect states to be attuned primarily to the risks that gaps in gains might entail for their military security, realist theory would lead us to expect states to focus on these risks *and* the risks that such gaps may pose for their relative bargaining power and long-term independence.

In sum, Powell's model supports the realist view that there is a strong link between international anarchy, military security, and the relative-gains problem for cooperation. However, aspects of his model diverge from realist views about the range of security-based state concerns about relative gains. In addition, Powell and I disagree as to whether states may have concerns about relative gains that are immediately connected not to their military security but, instead, to their relative bargaining power and independence in world affairs. These divergences about the scope and sources of state concerns about relative gains lead to different expectations about the prevalence of the relative-gains problem for cooperation. At least in regard to the question of the scope of relative-gains concerns based on security calculations, the differences in expectations about the incidence of the relative-gains problem are due to dissimilarities in the level of uncertainty that states are assumed to experience and the associated amount of risk that states are assumed to be willing to accept as they evaluate opportunities to cooperate. What results, then, from a juxtaposition of Powell's model and the realist perspective is a highlighting of the likely importance of risk and uncertainty as factors influencing state concerns about gaps in gains and, consequently, the incidence and severity of the relative-gains problem for cooperation. Further analytical and empirical investigation of these issues is very likely to produce extremely interesting results. Thus, Powell's essay helps identify important research issues and thereby advances the study of the problem of international cooperation while providing additional analytical support for the realist position.

CONCLUSION

Two conclusions may be derived from this discussion. First, Powell's essay shows one possible avenue toward progress in international relations theory. Powell shows that states may have rational grounds for being concerned about the division of otherwise mutually positive gains from cooperation, and that such concerns might impede cooperation. At the same time, the assumptions in his model concerning state risk and uncertainty differ from realist views, but this encounter of ideas highlights the likely importance of these factors in affecting the incidence and severity of the relative-gains problem for cooperation. Powell's engagement with realism is therefore helpful in two respects: it helps show that the realist approach is on the right track, and it helps point to important lines of future research on the problem of international cooperation.

Second, we can draw an important lesson from the problems that afflict Snidal's engagement with realist theory. If we want to argue that a theory in international relations or any other field is defective because its assumptions can be employed in such a way as to yield results contrary to those currently offered by the theory, then it is important that we use the assumptions that are actually contained in the theory under examination. Of course, if we want to criticize a theory by arguing that the assumptions contained in another approach are more powerful, then we should feel free to use whatever that alternative set of assumptions might be. However, we are unlikely to advance the cause of meaningful scholarly discourse if we assert that we are going to use the assumptions of a theory in order to criticize it, while actually relying heavily—indeed, crucially—on assumptions that are precisely the opposite of those specified in that theory. At best this will result in confusion; at worst it will delay the development of truly effective critiques and hamper the development of superior theory.

JOSEPH GRIECO

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RESPONSE

In his discussion of my essay, Grieco raises one major concern. He believes that my specification of "the range of causes of state concerns about relative gains" (Powell 1991, 2) is too narrow. Whereas my analysis emphasizes the importance of the potential use of force as the source of this concern in structural realism, Grieco says: "Realists would argue that there are at least two additional sources of state concerns about relative gains: uncertainties about one's partners and the efficacy of force and fears about the nonmilitary consequences of gaps in gains. They would argue that this wider range of sources of state concerns about gaps in gains causes the relative-gains problem to come into operation more frequently than might be expected on the basis of Powell's model" (p. 733). I want to respond to this concern in three ways: (1) by reiterating what the primary goal of my article was, (2) by addressing Grieco's concern more directly, and (3) by discussing a broader and more important issue that this controversy may tend to obscure.

The primary goal I sought to achieve in my article was very simple. My reading of the growing debate in the international relations literature over the problem of absolute and relative gains was that the assumptions that one made about a state's preferences were becoming something of a litmus test for distinguishing between neoliberal or structural realist analyses. In particular, "neoliberals assume that states have only one goal in mixed-interest interactions: to achieve the greatest possible individual gain" (Grieco 1988a, 496). States "do not gain or lose utility simply because of the gains or loses of others" (Keohane 1984, 27). In contrast, "realism expects a state's utility function to incorporate *two distinct terms*. It needs to include the state's individual payoff, . . . reflecting the realist view that states are motivated by absolute gains. Yet it must also include a term integrating both the state's individual payoff . . . and the partner's payoff . . . in such a way that gaps favoring the state add to its utility while, more importantly, gaps favoring the partner detract from it" (Grieco 1988a, 500; emphasis original).

This litmus test seemed to me to do two things. First, it focused attention on states' preferences rather than on a more careful specification of the constraints that face the states and thereby define the strategic environment in which they interact. For the reasons I shall outline, I believe that this emphasis is misguided. Second—and more importantly for the logic underlying my article—the litmus test seemed to be asserting that one could not construct a model that simultaneously satisfied two conditions: (1) that the model's implications regarding the feasibility of international cooperation would be in keeping with structural realism and (2) that the states would be assumed only to be trying to maximize their absolute gains. The goal of my article was to present a counterexample to this claim by constructing a model that satisfied these two conditions. I hoped that such a counterexample would show that whatever implicit and unarticulated assumptions distinguish the neoliberal institutional and structural realist conceptualizations of the international system, the distinction between these different conceptualizations does not turn on whether one assumes states to be maximizing their absolute gains. Clarifying this distinction and assessing its empirical import seem to me to be open questions in international relations theory.

Two criteria guided the design of the counterexample. First, I wanted to anchor it as closely as possible to the existing formal discussions of the problem of absolute and relative gains. That consideration meant using the repeated prisoner's dilemma as a point of departure. Second, simpler counterexamples are generally better than more complicated ones, because it is often easier to see in the former than in the latter where the assertion goes awry. To this end, I also tried to construct the simplest possible counterexample.

Turning more directly to Grieco's concerns, in my view he is correct to say that my model (or at least the substantive interpretation of it that I emphasize in my article) excludes some of the sources of states' concerns about relative gains that realists discuss, in particular, the nonmilitary consequences of relative gains. I have two comments to make in response.

First, my goal was not to try to develop a model that encompassed all of the sources of concern identified in structural realism. It was, rather, to show in the simplest possible formal setting that one could construct a model whose implications were generally in keeping with structural realism and in which the states were maximizing their absolute gains. I infer from Grieco's comment that my model "confirms the basic realist argument that states in anarchy may have security-based concerns about the distribution of gains from cooperation" (p. 729) that he believes that the implications of my model are generally in keeping with structural realism. However, the *argument* inherent in the analysis of my model is fundamentally *inconsistent* with Grieco's formulation of "the basic realist *argument*": the states in my model are solely maximizing their absolute gains and such a utility function is incompatible with the realist expectation or assumption that a state's utility function must "incorporate *two distinct terms*," one of which reflects the state's concern for relative gains (Grieco 1988a, 500; emphasis original). My model is not intended as a counterexample of realism's conclusions. (Indeed, the model was deliberately designed to support those conclusions.) Rather, it is intended as a counterexample to the assertion that one needs to assume that a state's utility is at least partly a function of relative gains in order to sustain the conclusions generally associated with structural realism.

Second, Grieco suggests that my model excludes the "nonmilitary consequences of gaps in gains" (p. 733). The formalism of a model may often be given many different substantive interpretations. The repeated prisoner's dilemma, for example, may be interpreted as a model of arms races as well as of international banking (Oye 1986). My essay developed a particular substantive interpretation that underscored the importance of the potential use of force. I stressed this because I was trying to establish that the conclusions that could be inferred from the model were generally in keeping with structural realism, which has often focused on the threat of force in the international system. Stressing this interpretation was an important step in establishing the model as a counterexample. But as I said in the article:

> By emphasizing that structural realism implicitly assumes that the nature of military conflict means that the potential use of force creates a set of constraints in which states can exploit their relative gains, the preceding discussion may have obscured a more general point. The general problem confronting a state in this system is one of constrained optimization in which the units are trying to maximize their absolute level of economic welfare subject to a set of constraints in which a unit's current relative gain may be translated into a future absolute gain for that unit and a future absolute loss for the other units. If this more general description also applies to other systems, then the present analysis may also help to explain why cooperation may be difficult to achieve in those systems even though the units in those systems are attempting to maximize their absolute gains and the possible use of military force is not a relevant part of the

> problem. If, for example, long-run profit maximization within an oligopolistic market structure depends on relative gains in terms of, say, market share, then this analysis would suggest that it may be very difficult to achieve international cooperation and collaboration even if national security concerns are not at issue. Describing the problem faced by states in the international system in the more abstract terms of the optimization problem confronting them may suggest previously unappreciated parallels between this and other problems where the possible use of force is not the source of the concern for relative gains. (Powell 1991, 1315–16)

In other words, the basic point of the counterexample seems likely to apply in many substantive contexts where the use of military force is not at issue. The point is that the problem of joint or cooperative action among states or firms or other kinds of actors may be studied in many different substantive settings (including those generally associated with structural realism) by embedding the assumption of absolute-gains maximization in games that model more carefully the constraints facing the actors. If those constraints create the kind of optimization problem I described, then we might expect the actors to exhibit a concern for relative gain regardless of their substantive setting.

Although this basic point seems quite general, the model I developed (Powell 1991) is too simple and too contrived to do much more than serve as a counterexample. As noted, the model was in some sense designed backward. I began with the conclusions I wanted to support (i.e., those of structural realism) and worked backward to try to support them with the assumption of absolute-gains maximization. Such is the logic of counterexamples.

But the counterexample does serve another purpose. It clears the ground for richer, less contrived models that embed the assumption of absolute-gains maximization in a more detailed specification of the constraints facing the states. My effort to formalize the classic guns-versus-butter problem is an attempt to develop a richer model (Powell 1993). Although this model is richer than my counterexample, it also leaves much out, including the two additional sources of a state's concern for relative gains that Grieco mentions. To the best of my knowledge, none of the existing formal work in international relations theory, including the prisoner's dilemma that Grieco and others have used to analyze the problem of relative gains, does a good job of modeling these sources. Their formalization remains a challenge for future work.

Finally, I want to discuss a broader issue that is implicit in this controversy and, more generally, in the debate about relative gains in the international relations literature. As I have discussed in more detail elsewhere (Powell 1993), much of the most influential work in contemporary international relations theory draws more or less explicit analogies between the international system and oligopolistic markets (e.g., Gilpin 1981; Keohane 1984; Waltz 1979). But with few exceptions, international relations theory lacks counterparts to the formal models that exist in economics. Providing those counterparts is an important outstanding problem in international relations theory.

But the development of formal, game-theoretic structural models in which states are taken to be rational, unitary actors poses a difficult question. We must make some assumption about state preferences. Should we assume that states solely maximize absolute gains or that their utility is at least partly a function of relative gains? The logic underlying the counterexample does not answer this question. The counterexample shows that the assumption of absolute-gains maximization is quite compatible with a desire to study issues (e.g., the effects of the shadow of force on international politics) that are generally seen as central to structural realism. It may, however, still be possible to develop models based on the assumption that a state's utility partly depends on relative gains. The counterexample does not preclude this possibility.

The flavor of the litmus test is that there is an a priori answer to the question of what to assume about states' preferences. That is, we can somehow deduce which assumption to make from some first principles. Accordingly, Grieco "attempts to demonstrate that realism is *logically* superior to neoliberalism" (1990, 11; emphasis mine). The counterexample indicates that such a demonstration cannot be developed, at least in the context of the prisoner's dilemma that Grieco (1988a, 1988b, 1990) uses as the foundation of his argument, because essentially the same conclusions can be derived from my model as from his modification of the prisoner's dilemma in which states' preferences are defined over relative gains.

I do not believe that there is an a priori answer to the question of what to assume about states' preferences. Once we have assumed states to be rational, unitary actors, we are at such a high level of abstraction that the question of preferences becomes one of usefulness and insightfulness. Which assumption about states' preferences in combination with which other assumptions about the strategic environment in which the states interact will yield the most useful and insightful models? This is an empirical and pragmatic question about the potential fruitfulness of different ways to model phenomena. It is a question that can be addressed only after we have more models. Based on the usefulness of the assumption of profit maximization in economics and some first efforts in international relations theory (e.g., Brito and Intriligator 1985; Kim and Morrow 1990; Powell 1993), my intuition is that assuming states to be maximizing absolute gains and investing our energies in more careful specifications of the constraints that define the states' strategic environment will prove the more fruitful and insightful course. But this is only an intuition.

ROBERT POWELL

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RESPONSE

Joseph Grieco incorrectly believes that the argument in my paper (Snidal 1991a) depends critically on special assumptions such as constant returns to scale, equality of payoffs or symmetry. In fact, the central result that bothers him is driven solely by the fact that increasing numbers of actors (n) change the strategic incentives created by relative-gains *seeking*. That is what I claim; it should have been readily apparent to Grieco given that my model replicates his argument regarding the inhibiting effects of relative gains among small numbers of actors even as it shows how these effects diminish as n increases. Since only the number of actors—not auxiliary assumptions—change between the two cases, it is clear that the result is not driven by these assumptions but by the impact of n. So that there can be no doubt on this point, I shall demonstrate formally that this result is unaffected in a variation of my model that allows for unequal payoffs.

Grieco's misunderstanding goes beyond the technical results and reflects a deeper substantive misunderstanding of the relative-gains problem itself. He fails to appreciate that the relative-gains argument depends on states' motivations, not on assumptions that states achieve unequal gains. This misunderstanding leads him to focus heavily on what is a very secondary and nonessential part of my model. Grieco's discussion of related issues such as the relationship between relative gains and cheating, Robert Powell's (1991) article, and how to think about the number of actors in the system, is equally confused.

Finally, I conclude with a comment on how the ongoing relative-gains debate reflects on theorizing in international relations more generally. My basic point is that, while they have their differences, neoliberalism and neorealism are such closely intertwined theories that more is lost than gained by treating them as diametrically opposed approaches. Whether or not states seek relative gains, scholars would do better to pursue the absolute gains possible by drawing on both perspectives rather than the relative gains of a (Pyrrhic) victory for either perspective.

THE IMPORTANCE OF RELATIVE-GAINS *SEEKING*

The key to the relative-gains argument is the claim that states *pursue* relative gains, regardless of whether any state ever *achieves* relative gains.^{7} Relative-gains seeking is important because it changes the structure of incentives among states regardless of the specific configuration of absolute, and therefore relative, payoffs (Snidal 1991a, 703–4). In particular, relative-gains seeking increases the incentives of states to break cooperative agreements in small-n situations, whether or not those agreements provide equal benefits to the parties (see also Powell, 1991, 1313). In large-n situations, however, relative-gains motivations do not affect states' incentives in this way, whether or not agreements provide equal benefits. Thus the key impact of relative gains as a motivation does not depend strongly on the details of particular cooperative arrangements, although it is strongly affected by other factors such as n.

This distinction between the pursuit and the attainment of relative gains is a variant of the familiar observation that, in social settings, individual motivations are not necessarily reflected in either individual or collective outcomes. A relevant example is the perfectly competitive economic market, where entrepreneurs are motivated by the pursuit of profits but profits are never realized. Yet the absence of profits in equilibrium does not contradict the assumption that the pursuit of profits is the fundamental motivation of entrepreneurs. Similarly, in the prisoner's dilemma (which is analytically equivalent both to the entrepreneurial situation and to the relative-gains problem), actors seek to maximize their individual outcomes even though they fail to do so given the structure of their interaction. Finally, in the n-actor relative-gains equilibrium where none cooperate, no state achieves relative gains even though relative-gains seeking explains the absence of cooperation (Snidal 1991b). The fact that actors cannot achieve their goals does not impeach their underlying motivation. Therefore it is important to distinguish the pursuit of relative gains as a motivation, which is fundamental to the argument, from the presence of relative gains as an outcome, which is not fundamental.

Grieco's misunderstanding of this distinction weakens the relative-gains argument he seeks to defend. It leads him to the mistaken conclusion that the relative-gains problem can be dissipated by making cooperation more equal through altering the agreement or side payments (p. 730–731). He incorrectly thinks that I use this as a trick in my article to get my results. As I state and show, however, negotiating balanced cooperative agreements can eliminate relative *gains* in particular cooperative arrangements but cannot eliminate the impact of relative-*gains seeking*. Indeed, sometimes a less equal distribution of benefits will be more conducive to cooperation because of differential concerns for relative gains among participating states (as I discussed in the final section of my article). In other cases, there is no way to alter the distribution of cooperative benefits to overcome the relative-gains problem. Thus the relative-gains debate starts from a premise regarding the motivation of states, which should not be confused with general distributive questions that are equally important in standard absolute-gains analysis.

THE DECREASING IMPACT OF RELATIVE-GAINS SEEKING WITH INCREASING *n*

The Appendix shows in more detail that my central result is independent of special assumptions about the distribution of payoffs. Starting from any initial distribution of absolute and therefore relative payoffs, it examines the calculus facing two states presented with an opportunity for cooperation. This opportunity can involve any combination of costs and benefits of cooperation for them, and these costs and benefits can be different for each of them. Thus there is no assumption of constant returns, equality, or symmetry restricting the possible payoffs from bilateral cooperation. The result is that state *i* will abide by a cooperative agreement with state *k* provided

$$\phi_i > \frac{c_i + r\, w_{ik} b_k}{b_i + r\, w_{ik}\, c_k} \qquad (1)$$

where ϕ_i is state *i*'s discount factor, c_i and b_i are the absolute costs and benefits of cooperation to state *i*, c_k and b_k are the absolute costs and benefits of cooperation to state *k*, *r* is the weighting of relative-versus-absolute-gains concerns, and w_{ik} is the extent to which state *i* focuses its relative-gains concerns on state *k*. Provided the parallel condition also holds for state *k*, cooperative agreements are sustainable between them even though both are relative-gains seekers to the degree measured by *r*.

Equation 1 is identical to the condition that I reported in the original article except for the subscripts on the *c*s and *b*s (Snidal 1991a, eq. 4). This simplification, which is the only impact of the constant-returns assumption for the general argument, does not affect its interpretation. In a world with only two states that care greatly about relative gains with each other (i.e., high *r* and $w_{ik} = 1$), cooperation will be difficult or impossible, since the required discount factor must approach or exceed unity for one of the two states.[8] As the number of states goes up, we would generally expect w_{ik} to decrease since relative-gains concerns must now be distributed across more states. This means that the impact of relative gains is severely attenuated with larger *n*, regardless of the level of *r*. Indeed, as *n* gets large and w_{ik} approaches zero, cooperation is no more difficult than under pure absolute-gains seeking (i.e., the relative-gains condition in equation 1 reduces to the absolute-gains condition $\phi > c/b$), as I reported in my article. Grieco cannot pretend otherwise by pointing to the use of constant returns to scale since that is not a "key" to this argument.

The secondary consequences of variations in *c*s and *b*s across actors are not surprising, do not change the results significantly, and are clearly indicated in the notes to my article. As with absolute gains, states fortunate enough to face lower costs (c_i) or higher benefits (b_i) are more willing to cooperate. In addition, under relative gains, states whose partners face higher benefits (b_k) or lower costs (c_k) will find cooperation less attractive for the obvious reasons. It should be noted that a change in one's own costs and benefits is always more consequential for a state than an equivalent change in a partner's costs and benefits. In brief, adding the extra subscripts to the model does not substantially alter the conclusions or provide any support for Grieco's position.

Finally, while the result that upsets Grieco does not depend on it, I believe the constant-returns assumption provides a useful extension to the relative-gains discussion. Since Grieco argues that states are always concerned that others will gain more through cooperation, it is interesting to ask whether we should expect any systematic bias in the distribution of gains.[9] Constant returns produce the perhaps-surprising implication that cooperative gains will be independent of the size of actors. This means that variations in relative-gains concerns among states of different sizes will be driven not by expectations of different absolute gains but by the different relative-gains weights that states place on one another, perhaps because of their different sizes. I used the latter point to discuss broader implications of relative gains later in the article. It also reinforces the point that relative-gains seeking is the more consequential than relative-gains outcomes per se. Since Grieco does not like my assumption (although he doesn't justify his dissatisfaction), he might want to consider some alternatives. For example, if returns to cooperation are decreasing, then smaller states will gain more from cooperation than larger states. This may further attenuate the impact of relative-gains concerns since the states that gain more are the ones that threaten other states less. Of course, larger states might then hesitate in cooperating with smaller states; but then, clusters of the latter would cooperate among themselves and catch up even faster on large states that forgo cooperative opportunities. Although we need stronger theoretical and empirical knowledge on these points, careful investigation of their consequences would seem more productive than unsupported (and incorrect) allegations that particular assumptions are being used to produce particular results.

FURTHER CONFUSIONS IN GRIECO'S DISCUSSION

Unfortunately, Grieco's confusion extends beyond his misunderstanding of the subtleties of the relative-gains argument or his inability to interpret my model properly. First, Grieco emphasizes that relative gains is an alternative to *cheating* as an explanation of the failure to cooperate. In fact, the relative-gains argument is intertwined with considerations of cheating. Relative gains do their work by increasing the incentives to cheat while decreasing the benefits of cooperation. If there is no consideration of cheating, then agreements might sometimes be easier to reach under relative-gains considerations that narrow the acceptable bargaining range. As I shall discuss, Grieco's preoccupation with distinguishing his argument from liberal ones sometimes leads him to obscure such important complementarities.

Second, I agree with Grieco that Robert Powell provides an interesting analysis of the same general problem, although Powell does so in a way that is very detrimental to Grieco's own position. In his accompanying comment, Powell describes his argument as "fundamentally *inconsistent*" with Grieco's (p. 736). Powell assumes that states are motivated by absolute gains and his goal is to show that realism does not depend on arguments like Grieco's. Thus relative-gains seeking is not fundamental in his model but strictly derivative of the pursuit of long-run absolute gains under certain constraints (Powell 1991, 1302–4; Snidal 1991a, 704). Furthermore, Powell points out that Grieco's (1990) empirical analysis of nontariff barriers is not relevant to arguments about states as relative-gains seekers since it is "a context in which it is difficult to attribute any concern for relative gains to the effects that a relative loss may have on the probability for survival" (1991, 1319, n. 26).

Powell provides an interesting analysis of when security concerns inhibit cooperation between states concerned with maximizing absolute gains over the long run. He also raises an important question as to the applicability of iterated game models like mine to extreme situations where national survival is at stake from one period to the next (i.e., where unilateral defection in one period enables a state to defeat the other militarily in the next period). This distinction is consistent with liberal arguments (Lipson, 1984), and Powell develops it in a way that is nicely integrative of realism and liberalism rather than mutually destructive. Any conclusion that relative gains inhibit cooperation is restricted to these extreme situations, however, and Powell's argument does not show cooperation infeasible in the less extreme (and more typical) circumstances where national survival is not directly at stake.

Thus Powell's model is not really about the sort of cooperative interactions that Grieco is examining. A fundamental assumption in the realist variant of Powell's model is that a one-period unilateral defection is sufficient to create a decisive military advantage between roughly equal states. Although Powell presents this as a trade example, it is hard to imagine that there has ever been a trade agreement of such monumental importance over the relatively short time frame required to react to the other's initial defection. Powell speculates that his result might obtain even where the use of force is not so proximate (1991, 1315–16), but this possibility awaits demonstration. Moreover, the model involves only two actors and so does not address the issue of changing incentives as n increases. This problem is much more complicated because it involves a specification of factors (e.g., balancing behavior) that need not be addressed in Grieco's argument. It is nevertheless possible to see why increasing n might have a similar impact in decreasing the noncooperative incentives facing states, although the exact circumstances would need to be specified and examined more carefully. For example, if states follow balancing behavior, this would offset the ability of one state to gain a decisive advantage over another state through unilateral defection. In addition, states now face the fear not only that they are vulnerable to defection by a cooperative partner but also that, if they fail to cooperate, they are vulnerable to other states that cooperate with each other (Snidal 1991b). The calculus is much more complicated, but it is important for states (and scholars) to work it out carefully rather than simply presume that noncooperation is the best policy, especially since the intuition suggested by my paper is that cooperation becomes more attractive as n increases. Thus Grieco may like Powell's article (as do I) but he should not fool himself that Powell's analysis supports his own position.

Third, Grieco's discussion of the *number* of states in the system is misleading. He is pedantic in his observation that since there are hundreds of states in the postwar system, only my large-n results should pertain. After all, his realist tradition has discussed the Cold War period largely in terms of bipolarity (i.e., $n = 2$) because it realizes that blind head-counts of states are meaningless without careful attention to their relative significance in the system and to each other. Contrary to his claim that I do not incorporate such considerations in my argument, the w_{ik} coefficient is explicitly an index of such factors and is central to the analysis (see equation 1 and Snidal 1991a, 716). I also make clear that asymmetries among states have the effect of reducing n in terms of understanding the results (pp. 719–721). Interpreting my model does require some subtlety, but it is repaid with a better understanding of the impact of the relative-gains argument.

Finally, Grieco's discussion of my final section on the implications of relative gains is so distorted that I can only ask readers to return to my paper to judge for themselves. I offer two points of guidance. First, I present these discussions as informal sketches and am explicit about how I am using the model as a baseline against which to interpret a historical experience that is obviously far richer than can be captured in any simple model (Grieco's or mine). Second, I pay special attention to a limitation of my model found in my specification of the relationship between relative-gains weight and "size." I suggest an informal amendment to this specification which provides a *plausible* reconstruction of certain postwar trends and actually strengthens the usefulness of the relative-gains hypothesis over more vulgar versions that pay no attention to such factors. There is no doubt that my discussion here is speculative, but it is speculation guided by a very precisely specified model that sets useful limitations on the claims that can be made. I commend the technique to Grieco.

LET'S HALT THE PHONY REALIST–LIBERAL DEBATE (OR, CAN THERE BE MORE TO IR THEORY THAN MUTUAL ASSURED DESTRUCTION?)

Grieco's fundamental problem is that he is too concerned with "winning" the great international relations debate by vindicating realism from my allegedly unfair attack and thereby defeating liberalism. This is partly understandable in that a strength of Grieco's work has been lining up and contrasting the assumptions and conclusions of the two approaches. There is sometimes significant value in drawing such sharp contrasts between theories, thus clarifying different arguments and identifying key points of contention. But overemphasizing differences between the approaches obscures the many similarities in their fundamental assumptions, including anarchy, states as the actors, and goal-seeking behavior. Not surprisingly, such closely related theories often come to very similar conclusions, including about the possibilities for cooperation under anarchy.

Moreover, Grieco has severely biased the terms of the debate. Probably the most important example is his argument that realists believe that states seek *both* absolute and relative gains whereas liberals believe that states seek *only* absolute gains. Under this specification, the realist model subsumes the liberal model as a special case and therefore cannot "lose" the debate by construction. This is not an isolated example since in Grieco's comparison of realism and liberalism, six of the eight realist assumptions include the corresponding liberal assumptions as a special case (1990, 49). Furthermore, Grieco treats disagreements over the distribution of cooperative gains as evidence of relative-gains seeking, even though such disputes are equally well explained by tough bargaining in a purely absolute-gains world. Finally, Grieco seems to judge arguments as "realist" according to whether he likes their conclusions rather than according to their theoretical presuppositions. Since he likes Powell's conclusions, he largely endorses Powell's analysis even though Powell diverges substantially from Grieco's line of argument. Powell also assumes that states make equal gains through cooperation, but Grieco does not object to the same assumption that he finds so troublesome in my model. By contrast, because he does not like my central conclusion, Grieco rejects my argument even though (unlike Powell) I accept his premise that states seek relative gains as the fundamental starting point for my analysis. And when Grieco likes some of my conclusions, he infers that I must have abandoned my own argument and smuggled in a realist one in order to reach them.

It is hardly surprising that some of my conclusions seem "realist" since my model is explicitly a model of the realist relative-gains argument, beginning with a specification for relative gains that is analytically equivalent to Grieco's. My analysis is biased in favor of realism both in accepting the assumption that states seek relative gains and in the use of a prisoner's dilemma specification that I earlier showed to be the most favorable assumption for the relative-gains case. Moreover, my conclusions are supportive of many realist positions. I point out that the relative-gains argument has significant bite in small-*n* worlds and the last part of my paper teases out plausible implications of the relative-gains argument for understanding important international trends. Of course, I also point out that the relative-gains argument has a limited domain, produces possible inconsistencies with other realist arguments, and is not the master weapon with which realists can finally destroy all liberal arguments about cooperation. Surely careful specification of the contexts in which relative gains are (and are not) important strengthens realist theory regardless of whether it advances particular conclusions that Grieco believes are properly "realist."

Grieco's comment is symptomatic of an increasingly phony and destructive debate in international relations between "neoliberals" and "neorealists." The two perspectives share much in common and reach similar conclusions regarding many matters. Even when dealing with contentious issues such as relative gains, it is more productive to investigate how the different arguments work together than to seek to establish either liberalism or realism as a clear victor. Both Powell's and my paper are in this spirit. Powell brings liberal and realist arguments together in his model and shows that each applies under different conditions regarding the technology and cost of using force. I show that relative-gains seeking impedes cooperation in small-*n* worlds but has a decreasing impact as the number of actors increases. These are not "victories" for either side but each takes us a step toward a contextually richer theory that is able to explain international politics better than either vulgar realism or vulgar liberalism in isolation.

APPENDIX

It is straightforward to show that my result does not depend on special assumptions regarding constant returns to scale, symmetry, or equal costs and benefits of cooperation across actors. The notation follows my earlier and fuller presentation of the argument (Snidal 1991a).

Let n states be represented by their initial circumstances $\{P_1, P_2, P_3, \ldots, P_n\}$, where P_i is the payoff to state i in absolute terms. The ith state's relative-gains payoff is

$$P_{ir} = (1-r)P_{ia} + r\sum_{j=1}^{n} w_{ij}(P_{ia} - P_{ja}), \qquad \textbf{(A-1)}$$

where $0 \leq r \leq 1$ represents the weight that states place on relative gains and $0 \leq w_{ij} \leq 1$ indicates the emphasis that state i places on state j's performance in evaluating its relative-gains position. We assume that $\Sigma_i\, w_{ij} = 1$ for the reasons discussed in Snidal 1991a.

Suppose that states i and k face an opportunity to cooperate in an ongoing interaction where cooperation costs a state c_i (c_k) and a state benefits b_i (b_k) from the other's cooperation in each period. Provided $b > c$ for both states, there are absolute gains to be made from mutual cooperation. This situation corresponds to an ongoing prisoner's dilemma. (I show why the prisoner's dilemma has pride of place in relative-gains theorizing and why my use of it is biased in favor the realist relative-gains argument in Snidal 1991a, 704–14.) A cooperative equilibrium can be sustained in an absolute-gains world (i.e., $r = 0$) provided $\phi > b/c$ for both states, where ϕ^t is the discount factor applied to benefits received t periods into the future.

When a state also cares about relative gains, its calculations are more complicated and must include comparison of its position to the positions of other actors in the system. If *i* and *k* cooperate through time, then *i*'s relative gains payoff will be

$$P_{ir} + (1 - r)\frac{b_i - c_i}{1 - \phi_i} + r\frac{b_i - c_i}{1 - \phi_i}\sum_{j \neq k} w_{ij} + r\, w_{ik}\frac{(b_i - c_i) - (b_k - c_k)}{1 - \phi_i}. \qquad \textbf{(A-2)}$$

The first three terms correspond closely to Snidal 1991a, equation 2, and represent (1) the value of the preexisting situation shown in equation (A-1) (2) the absolute-gains value of cooperation to state *i*, and (3) the relative gains that state *i* achieves over third-party states through cooperation with *k*. The fourth term reflects relative gains made by *i* over *k* through the proposed cooperative arrangement. Note that the addition of this term is the biggest change in the model and (as I shall show) is not terribly consequential.

Next consider state *i*'s relative gains payoff if it defects from this arrangement, recognizing that this will lead to the breakdown of cooperation between the two states:

$$P_{ir} + (1 - r)\, b_i + r\, b_i \sum_{j \neq k} w_{ij} + r(b_i + c_k)\, w_{ik}. \qquad \textbf{(A-3)}$$

All four terms correspond closely to in Snidal 1991a, equation 3, and represent (1) the value of the preexisting situation shown in Equation A-1 (2) the absolute-gains benefits of a free ride on the other's cooperation, (3) the relative gains over third parties achieved through unilateral defection, and (4) *i*'s relative-gains incentives, vis-à-vis *k*, to break the agreement.

The viability of cooperation requires that the incentives for *i* to cooperate in equation A-2 exceeds its incentives to defect in equation A-3. This means that state *i* prefers cooperation provided equation 1 holds

$$\phi_i > \frac{c_i + r\, w_{ik} b_k}{b_i + r\, w_{ik}\, c_k}.$$

A parallel condition holds for state *k*. Both these expressions are *identical* to Snidal 1991a equation 4, except for the subscripts on the *c* and *b* terms, and the basic result follows without any appeal to special assumptions contrary to Grieco. In particular, w_{ik} goes to zero as *n* increases, and the condition for relative-gains cooperation approaches the condition for absolute-gains cooperation. The text discusses the secondary implications of this change in treating the two actors.

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Notes

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1. Modern realists include Kenneth Waltz (1979, 1986), Robert Gilpin (1975, 1981, 1987), and Stephen Krasner (1976, 1978, 1985, 1991). Neoliberal institutionalists include Arthur Stein (1983), Charles Lipson (1984), and Robert Keohane and Robert Axelrod (Axelrod 1984; Axelrod and Keohane 1985); Keohane 1984.

2. Snidal thinks that realists argue that the relative-gains problem results from states seeking to *maximize* gaps in gains *to their own advantage*. Yet, as can be observed of Waltz's (1979) statement of the problem quoted on p. 000. it is a defensively positional concern that partners might do better—not an offensively oriented interest in doing better oneself—that drives the relative-gains problem for cooperation. Snidal makes the same error in his essay (Snidal 1991b).

3. Snidal assumes in the two-country model that "the game is symmetric. . . . This means that two states are equally well situated to benefit or hurt one another and fashion their cooperative arrangement to provide equal absolute gains" (1991a, 705). This assumption of equal gains, as discussed in the text in regard to the large-*n* model, is the opposite of that specified in realist theory as generating the relative-gains problem. Therefore, one cannot, in contrast to Snidal's claims, find support for realism in his two-state model.

4. Snidal observes that I have argued against a perfect-symmetry assumption "because it entails equal payoffs from mutual cooperation." He then says, "I have argued that this can be readily achieved by adjusting the terms of cooperation and is not central to the relative gains argument" (1991a, 723, 8). As I suggest in the text, realists would not accept so readily or so uncritically this assertion that symmetry in payoffs "can be readily achieved"; therefore they would say that it is exactly the question of unequal gains that is "central to the relative gains argument."

5. I then note, "However, as that common threat becomes less severe, a state's tolerance for gaps in gains favoring allies will probably decrease" (Grieco 1990, 46). I stress here the idea that states will be more concerned about gaps in gains as external threats subside. Yet, I am not suggesting that such concerns will cause cooperation to fail, but, instead, that they will become increasingly salient.

6. This point is also discussed by Arthur Stein (1984) and Robert Gilpin (1975).

7. Grieco sometimes argues that states do not seek relative gains so much as avoid relative-gains losses (i.e., "defensive positionality"). The objective function that he proposes and I use, however, assumes relative-gains seeking. Grieco never clearly works out the implications of the seemingly weaker positionality assumption and it appears to be a distinction without a difference. In any event, my results would apply equally in this less nasty variant of the relative-gains world.

8. If $r = 1$ and $w = 1$, for example, then either $c_i + b_k \geq b_i + c_k$ in which case $\phi_i \geq 1$ and state *i* will not cooperate, or else $b_i + c_k \geq c_i + b_k$ so that $\phi_k \geq 1$ and state *k* will not cooperate.

9. The alternative, of course, is that there is no systematic bias in the distribution (i.e., no expected relative gains) but random fluctuations from period to period or issue to issue. Grieco frequently makes references to such fluctuations, in terms of risk or uncertainty, as inducing relative-gains concerns. Such considerations matter equally in absolute-gains analysis and so do not provide the distinctive theoretical position that Grieco is seeking to establish. Moreover, if they are central to the argument then they need to be more explicitly and carefully incorporated into it than has been the case to date.

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PARTY PLATFORMS, MANDATES, AND GOVERNMENT SPENDING

In their 1990 Review *article, Ian Budge and Richard Hofferbert analyzed the relationship between party platform emphases, control of the White House, and national government spending priorities, reporting strong evidence of a "party mandate" connection between them. Gary King and Michael Laver successfully replicate the original analysis, critique the interpretation of the causal effects, and present a reanalysis showing that platforms have small or nonexistent effects on spending. In response, Budge, Hofferbert, and Michael McDonald agree that their language was somewhat inconsistent on both interactions and causality but defend their conceptualization of "mandates" as involving only an association, not necessarily a causal connection, between party commitments and government policy. Hence, while the causes of government policy are of interest, noncausal associations are sufficient as evidence of party mandates in American politics.*

In a recent article in this journal, Ian Budge and Richard Hofferbert (1990) analyze the relationship between U.S. party programs and federal government expenditures. Their purpose is to assess what they describe as "the doctrine of the party mandate, according to which the competing parties offer to the voters different government programs between which they can choose. The party which attracts the most votes on this basis then forms the government, but it is bound (both morally and by fears of retribution at the next election) to carry through the program on which it has been elected" (p. 111).

The authors operationalize party policy in particular issue areas using a content analysis of policy "emphases" in party electoral programs, described extensively by Budge, Robertson, and Hearl (1987). Their basic intention is to show that government spending in particular issue areas is related to party policy thus measured. They do this by specifying and estimating a series of linear regression models.

Budge and Hofferbert state their conclusions very clearly. They talk of the "remarkable success" of one of their models and a "stratospheric" *R*-squared associated with another (1990, 122). This leads them to conclude that "party government in the United States works largely as mandate theories say it should" (p. 130). This is a remarkable claim, especially given that decades of research on political parties has demonstrated again and again that American parties are more diffuse, porous, and less programmatic than those in most other countries (see Epstein 1986 and the citations therein). Strong evidence that the mandate theory applies to the United States would mean that hundreds of observers of American politics were wrong. It would not be the first time, but it would be very surprising.

In what follows, we do not evaluate the reliability or validity of Budge and Hofferbert's data, the general justification of their models, or the statistical techniques used to estimate them. Instead, we make three more-specific points. First, with a few trivial exceptions, we find that Budge and Hofferbert's results can be replicated very easily. This is no minor achievement for the authors, inasmuch as it is often difficult or impossible to replicate empirical research published in the social sciences (see Dewald, Thursby, and Anderson 1986). Second, we show that the form in which Budge and Hofferbert present their analyses makes it impossible to evaluate their key hypothesis, because they do not provide enough information to calculate the appropriate standard errors or test statistics. We present their original results in an improved format, along with the corresponding correct standard error. Our re-presentation of their results indicates that fewer than half of their tests of their preferred specification support their hypothesis. Finally, we show that the few stratospheric coefficients that these authors report are to a large extent a product of their implicit implausible assumption that a new party achieves every budgetary desire immediately in its first budget cycle. We show that under more plausible and empirically verifiable assumptions, only a few of the many analyses support the authors' hypotheses. Moreover, even these rare positive results imply relatively modest causal effects, consistent with prior literature. This methodological improvement may also explain the anomalous signs associated with the coefficients in several of Budge and Hofferbert's published models.

METHODOLOGY

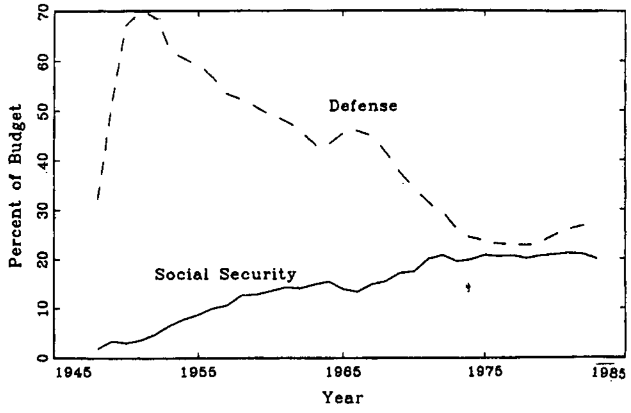
We focus on Budge and Hofferbert's most favored model for each of the 12 policy areas in their study. This is "Model 5, the general program model, positing dominance by the platform of the occupant of the White House" (1990, 129). Budge and Hofferbert specify this for each policy area as follows:

$$E(Y) = \beta_0 + \beta_r R + \beta_d D + \beta_{pr} RP + \beta_{pd} D(1 - P), \qquad (1)$$

where Greek letters are parameters to be estimated; *Y*, the percentage of federal government spending in a policy area two years ahead (the first time a new president can influence the budget);[1] *R*, the percentage of the Republican platform devoted to a policy area matched to the spending area; and *D*, the same percentage for the Democratic party platform. *P*

FIGURE 1

Trends in Federal Spending



Source: Budge and Hofferbert 1990.

equals 1 for Republican presidents and 0 for Democratic presidents.[2]

In their tables 3–6, Budge and Hofferbert present estimates of the parameters of equation 1 in each policy area. The theoretical focus of their article is the effect of the party platform of the winning presidential candidate on the federal budget expenditures. Thus, the key parameters of interest are $(\beta_r + \beta_{pr})$ for Republican presidents and $(\beta_d + \beta_{pd})$ for Democratic presidents.[3] One can easily obtain these point estimates from their article by addition, but the standard errors necessary for evaluating these point estimates cannot be derived from their tables. Indeed, Budge and Hofferbert's article provides no appropriate assessment of the uncertainty of their most important empirical evidence.[4]

We also address a serious problem in their analysis of these data—their explicit decision to ignore the clear secular trends in most of the budget figures they analyze. Thus, their figure 1 describes a "hypothesized relationship between governing party . . . policy emphasis and annual spending" that is essentially one of trendless fluctuation in party programs, correlated with lagged spending patterns (1990, 118). The authors claim that "this relationship would hold whichever way the lines rotated in space, though for simplicity no long term upward or downward trend in either expenditures or emphases is shown" (p. 117). In practice, of course, there are some very pronounced time trends in the real, as opposed to the hypothetical, data. Figure 1 shows this for two of the major federal budget policy areas, defense and social security. The problem is that Budge and Hofferbert's statistical model fits neither their hypothetical figure (rotated in space or otherwise) nor their data. Their decision simply to regress spending against party policy incorrectly assumes that each observation is conditionally independent of all others.[5] That this independence assumption is false is easy to verify by calculating the Durbin–Watson statistics (or other tests of autocorrelation) from their original regressions; our reanalyses indicate a textbook case of strong autocorrelation. Indeed, regressions with trending variables, very high *R*-squared values, and bad Durbin–Watson statistics are exactly the situation that Granger and Newbold (1974) described when they coined the term "spurious regression."

In other words, the goal of their analysis is to study what would happen to the federal budget in a policy area that received more attention by a party (as measured in its platform) while everything else was held constant. The fundamental problem in evaluating Budge and Hofferbert's claim is finding instances of party platforms that differ in the amount of attention given to a policy area for which "everything else" is constant. Indeed, because federal budget figures trend so strongly, these instances do not exist, so that the problem, in these and most other time series data, must be solved with statistical controls (given that it is not possible to replay four years of a presidency while changing only party emphases). From a substantive perspective, this can be interpreted as failure to control for a key explanatory variable—the history of their dependent variable the year prior to the party convention. This variable easily meets the requirements for producing omitted-variable bias: (1) it is correlated with the included variables (party platforms), (2) it influences the dependent variable, and (3) it is causally prior to the key explanatory variable. Thus, all the coefficients estimated in their paper and replicated in columns 1 and 3 of our Table 1, are biased.

Substantively, autocorrelation is equivalent to tricking yourself into believing that you have more information than you really do. This is the case when knowing last year's budget figures account for a large part of this year's. Ignoring this problem causes them significantly to underestimate their standard errors. To illustrate, if the unit of analysis in their original specification is changed to the presidential administration, the regression based on these nine quadrennial observations produces *identical* coefficients to their results but much larger standard errors (since there is no variation in platforms within administrations). (Indeed, if their unit of analysis were changed to monthly or hourly budget figures, their coefficients would also be identical, but the standard errors would drop considerably.) They openly worry about part of this problem, which they call the "double barrelled data extender," but the problem is not with how the data are organized. The problem is entirely with their specification, which picks up no more information from their 36 annual observations than do the corresponding 9 observation quadrennial data set. Only a specification with some sort of time series process will find additional information in the "extended" data set.

Budge and Hofferbert discuss and then dismiss the idea of detrending their variables as a possible solution to this problem. They are probably correct in deciding not to detrend, but the lack of any solution leaves the problem they recognized unresolved. Fortunately, it is easy to correct for both nonstationarity and autocorrelation in these data. We do so now by adding only *one* extremely plausible assumption to their model, namely, that winning parties may not get everything they want all at once.

Budge and Hofferbert's idea is that political parties have a *preferred* level of budget spending in a policy area in year t, which we label as Y_t^P. This is determined by (and thus equal to) the right side of equation 1. Our additional assumption is that parties may only be able to adjust budget spending by a fraction α ($0 < \alpha < 1$) of what they want toward their preferred level on average in each year:

$$E(Y_t) = Y_{t-1} + \alpha(Y_t^P - Y_{t-1}).$$

This "partial adjustment model" shows that on average the actual level of spending in the first year a new president can influence the budget is equal to last year's spending plus an adjustment toward the winning party's preferred spending level. For clarification, this equation can be written as

$$E(Y_t) = (1 - \alpha)Y_{t-1} + \alpha Y_t^P,$$

which shows that expected spending is a weighted average of last year's actual spending and this year's preferred level. The situation where $\alpha = 1$ is exactly Budge and Hofferbert's assumption that a winning party definitely gets everything it wanted all at once in the first year. Our alternative assumption that winning parties may not get all they want in the first year is correct if α is less than 1, an assumption that we can test for. Finally, we can rewrite the equation once more by substituting for Budge and Hofferbert's definition of what constitutes the preferred level of spending

$$E(Y_t) = \beta_0^* + \beta_r^* R + \beta_d^* D + \beta_{pr}^* RP + \beta_{pd}^* D(1 - P) + \beta_y Y_{t-1},$$

where $\beta_y = 1 - \alpha$ (see equation 1) and where the asterisks are included to indicate that these coefficients may differ from .those in equation 1. Thus, by adding only the assumption that newly elected parties may not get all that they want in spending immediately upon taking office, we derive a model that requires adding lagged spending to Budge and Hofferbert's list of explanatory variables. This assumption is correct if $\alpha < 1$, which we can evaluate by testing whether $\beta_y > 0$. This model also eliminates the omitted-variable bias, most of the problems with autocorrelation and nonstationarity, and the "double barrelled data extender" problem with defining the unit of analysis.[6]

RESULTS AND CONCLUSIONS

We provide revised coefficients and standard errors in Table 1. The first two columns of numbers provide estimates and standard errors of the effect of Republican platform emphases when the Republicans win the presidency ($\beta_r + \beta_{pr}$) and Democratic platform emphases when the Democrats win the presidency ($\beta_d + \beta_{pd}$) for each policy area. Each of these numbers is interpreted in the same manner: for every one-percentage-point increase in the amount of platform content spent on a policy area, the coefficient is an estimate of how much the federal budget in this area will increase. Budge and Hofferbert say clearly that under their preferred model, there is "an assumed positive relationship between program emphases and expenditures when a party holds the presidency" (1990, 120). They hypothesize, more specifically, that ($\beta_r + \beta_{pr}$) and ($\beta_d + \beta_{pd}$) should be positive.

As is plainly clear from this table, only 10 of the 24 coefficients in the first two columns are two-or-more standard errors greater than zero. Thus, without any modification at all in the data, methods, or models from their original article, we find that Budge and Hofferbert's mandate theory fails in a majority of the policy areas they examined. As an example of how to interpret these results, a 10 percentage point increase in Republican platform emphasis on education should lead to about a 3.8% increase in the proportion of the federal budget devoted to education when the Republican wins (plus or minus the standard error of .09); the effect for Democratic presidents is about three times this figure.

The right side of Table 1 also presents the results of a reanalysis allowing for the possibility that newly elected parties do not get all that they wish immediately, and the differences between this and their uncorrected analysis are quite striking. First, note the rightmost column in this table, which presents the coefficients on the lagged value of the dependent variable for our revised model and hence a test of whether our additional assumption is correct. In all but possibly one case, the coefficients are clearly greater than zero, indicating that indeed, presidents do not achieve their preferred level of spending immediately. Columns 3 and 4 report the coefficients that are directly comparable in their specification to the ones in columns 1 and 2, respectively. Whereas 10 of the 24 coefficients in their original specification were at least two standard errors greater than zero, only 4 of 24 meet this same criterion in our corrected analysis. In addition, about 1.2 of these four "significant" coefficients would be greater than zero as a result of chance alone. In addition, the coefficient for Democratic platform defense emphases (one of these four) seem extremely counterintuitive. It indicates that an increased Democratic emphasis on defense raises the proportion of the budget devoted to defense in Democratic administrations about 15 times more than increased Republican platform emphases do when the generally more hawkish Republicans take the White House. Moreover, virtually all of the estimates from Budge and Hofferbert's specification are reduced substantially in ours, most commonly by about half.

We have shown here that the evidence that Ian Budge and Richard Hofferbert present in their article usually does not support the mandate theory that they favor. Moreover, a respecification of their model

TABLE 1

Effect of Winning Party Platforms on Federal Expenditures

ISSUE AREA	ORIGINAL SPECIFICATION REPUBLICAN PRESIDENT	ORIGINAL SPECIFICATION DEMOCRATIC PRESIDENT	REVISED SPECIFICATION REPUBLICAN PRESIDENT	REVISED SPECIFICATION DEMOCRATIC PRESIDENT	REVISED SPECIFICATION COEFFICIENT ON LAGGED BUDGET
Social security	1.923	−1.318	−.064	−.142	.893*
	(1.669)	(1.194)	(.379)	(.273)	(.037)
Health & medicare	3.426*	−3.285	.207	−.159	.965*
	(1.229)	(.879)	(.170)	(.132)	(.022)
Income security	−.047	−1.768	−.494	−.392	.874*
	(1.348)	(.964)	(.481)	(.358)	(.060)
Education & Employment	.381*	1.213*	.197*	.442*	.657*
	(.090)	(.086)	(.050)	(.101)	(.073)
Defense	−2.450	6.872*	.213	3.322*	.907*
	(1.083)	(1.828)	(.289)	(.537)	(.044)
International affairs	−.094	1.015*	−.020	−.284	.741*
	(.126)	(.213)	(.057)	(.165)	(.099)
Natural resources	.104*	.072	.090*	.041	.689*
	(.040)	(.068)	(.020)	(.035)	(.096)
Energy	.064	−.073	.025	−.051	.844*
	(.038)	(.037)	(.015)	(.014)	(.065)
Transportation	.294*	.162*	.006	−.037	.911*
	(.064)	(.061)	(.052)	(.043)	(.115)
Community rehabilitation	.225*	−.064	.056	−.019	.762*
	(.042)	(.041)	(.032)	(.024)	(.097)
Commerce & housing	.103	−.003	.084	−.011	.200
	(.071)	(.068)	(.075)	(.069)	(.185)
General government performance	.110*	−.196	.075	−.083	.441*
	(.050)	(.032)	(.045)	(.044)	(.152)

Note: Entries in the first four columns of numbers are estimated effects of platform emphases, when the specified party wins, on federal government spending in the given policy area. The first two columns are replicated results from Budge and Hofferbert 1990, calculated as $\beta_r + \beta_{pr}$ for Republican presidents and $\beta_d + \beta_{pd}$ for Democrats. Columns 3 and 4 are calculated the same way from a regression that also includes a lag of spending as an additional explanatory variable. When the coefficient on this lagged variable, which appears in the final column, is greater than zero, it confirms our alternative assumption that newly elected parties may not have all their influence on the budget in the first year. Throughout, standard errors appear in parentheses.

*denotes a coefficient that is at least two standard errors greater than zero.

to account for the possibility that presidents do not immediately get all that they want obliterates most of the remaining apparent effects. In short, the stratospheric coefficients and strong relationships reported by Budge and Hofferbert are to a large extent methodological artifacts of a failure to correct for the effect of secular trends in the data. Budgets almost always change incrementally, and any analysis of the relationship between political variables and budgetary allocations must be sensitive to the substantive and methodological effects of this pattern on the results.

Although this reanalysis is consistent with considerable prior research about the diffuse nature of American political parties, we do not wish to imply that there is no connection between party positions and federal priorities. Instead, we agree that the connection probably does exist: it is just far weaker and more subtle than could be perceived with these data and methods.

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We are grateful for the care brought by Gary King and Michael Laver to their critique of our article (Budge and Hofferbert 1990). That research has something very important to say about party democracy. The data open a new and important avenue of comparative research. At the same time, King and

Laver have performed a service in pointing to certain key methodological questions that, if left unattended, could quite possibly result in doubt about the findings. We conclude, however, that a consideration of their critique leaves largely undisturbed the original claim to having demonstrated the operation of a party mandate in the American national policy process.

Although King and Laver refer to several considerations, two are key to their critique: (1) the use and interpretation of interactive terms and (2) the need to incorporate a lagged dependent variable on the right side of the equation.

INTERACTIVE TERMS

Interactive terms can be confusing. Generally the confusion arises (as it did in this case) because the coefficients indicate the *relative* effect of a variable. For example, the equation for our primary model takes the form

$$Y = a - b_1R - b_2D + b_3(R \times P_r) + b_4(D \times P_d) + e,$$

where all variables are as defined in the original article. The b_1 and b_2 coefficients tell the reader the policy priority effects (perhaps we should say "translation rates") of the Republican and Democratic platforms when that party loses the presidency. The b_3 and b_4 tell the reader the *difference* in the translation rate of the Republican and Democratic platform, respectively, when that particular party wins *relative* to when it loses. Thus, the b_3 and b_4 terms allow us to address (with associated reliability tests via the respective standard errors) what difference winning makes in a party's ability to translate its platform. To wit, we find, in 13 of the 24 instances reanalyzed by King and Laver, winning matters for positive translations of a party's platform emphases into expenditure priorities. This, we think, is important.

At the same time, the original article did not consistently phrase the argument in a manner so as to sustain attention to the relative nature of these elements in the equations. Thus, one can compare the following two statements:

> The contrast in parameter estimates for the untransformed term (*R* or *D*) and the matching interactive term (*R* × *P*) or (*D* × *P*) shows the *relative* gain in programmatic impact from holding the presidency. (p. 120, emphasis added)
>
> Thus, when they are *out* of the White House, for example, for every percentage change in the proportion of the Republican platforms devoted to social justice themes, we could have expected a 7.3% shift in social security's share of federal spending in the opposite direction. But when the Republicans are *in* the White House, each 1% up or down on social justice is directly reflected in a 9.7% shift in social security's share of outlays. (p. 123, emphasis original)

The first statement is a reasonably clear and correct summary of how to interpret the interactive terms in the total equation. The second statement is misleading, in that it does not clearly reflect the *relativity* of the translation process. We should have been much more consistent in our rhetoric.

King and Laver chastise us for not reporting the standard errors for the combination of the (b_1 and b_3) and (b_2 and b_4) terms. These combined terms are estimates of the translation rates for the Republican (b_1 and b_3) and Democratic (b_2 and b_4) platform emphases into spending priorities, given that the particular party has won. Their results show that 10 out of 24 times, a party's platform emphases have a statistically significant and positive translation into spending emphases, given that it has won. The three instances of positive effects in our report and null effects in theirs are a consequence of our analysis' taking into account that losing would have a statistically significant and negative effect, which is erased but not turned positive when a party wins.

We must agree that King and Laver raise important questions. Still, two points are worth the readers' attention. First, while accepting their point of qualification, we do not see it at all as disconfirmation. In 13 instances, winning matters for the translation of a party's platforms within the model. And in 10 of those instances the party's platform has a net positive effect, given that it has won. Second, and more important, one must recognize that a positive coefficient for the winning party's favored policy does not necessarily mean that the net spending share *increases* when the party takes office. For that to be the expected outcome, the effect of a platform when a party wins must be greater than any negative effect associated with the losing party's platform. If the apparent policy *rejection* by the incumbents of their opposition party's platform emphases is sharper than the *acceptance* of their own, the sum may indeed be a net reduction. Often, the mandate appears to operate in both directions (i.e., a positive translation of the winning and a negative translation of the losing party's platform), with neither being more or less advantageous for mandate theory. The logic of the positive and negative facets of the mandate is fully explained and explored by Klingemann, Hofferbert, Budge, et al., in our forthcoming 10-country study.

LAGGED DEPENDENT VARIABLES

Contrary to King and Laver's caution, our research did not include a lagged dependent variable on the right side of the equation. Whether it should have done so depends (as do all answers to questions of research design and specification) on the purpose of the analysis. If the purpose had been to test whether one could identify the platforms as a causal agent of spending priorities, then attempts to control for the recent level of spending priorities would have been in order. Such was not the purpose, despite the causal language into which the text occasionally slipped (e.g., in the discussion of Figure 1 and the decision not to detrend). On the question of cause, the article is explicit: "It is enough for mandate theory that expenditures should *correspond* to emphases rather than there should *necessarily* be a cause and effect relationship between them" (p. 118, emphasis original).

King and Laver disagree and therefore offer a causal specification for testing the relationship between platforms and spending priorities. A byproduct of their specification is that the congruence of trends or cycles of policy and platform emphases is irrelevant to mandate theory. This is not true, however; the similarity in cycles and trends are quite important. Imagine the logical extreme of their argument. Under their specification it is possible for platform emphases and expenditure priorities to follow opposite trends while still yielding the statistical impression that the platforms have a positive impact. This could happen when the platform and policy *deviations* from such contrasting trend lines are in a similar direction for each interelection period. In order for this to be a version of the mandate thesis, a party would have to say to itself, "We're saying less about this policy than we said a couple of elections back, but a bit more than the decline in our trend; therefore, we should spend relatively more." This reflects no notion of the mandate thesis of which we are aware. Therefore, while suitable for a causal analysis of spending, King and Laver's specification is not acceptable as a specification for examining the mandate thesis.

Their specification also raises the possibility of a false negative for the substantive theory of party. If we control through the lagged dependent variable, we in effect provide a proxy for all past influences on policy. One of those past influences, in years 2–4, is the platform itself. Since any policy associated with the platform is expected to hold through a four-year period, this proxy includes the effects of the platform for every year but the one transition year in each quadrennium.

So where does the conjunction of our and their findings put our understanding? In several policy categories, the winning party's platform emphases correspond to spending priorities (our finding), and in almost all of those categories, the correspondence is the apparent consequence of similar trends and cycles (their finding). Because the similarity in trends must be retained as part of any statement of the mandate thesis, the mandate thesis stands. Thanks to King and Laver, we know that it stands as a consequence of the trends, not just in part, but almost in its entirety.

The key question for the research is, Does government policy move in the direction indicated by the platforms? This is the key question of mandate theory. The research finds that the administrations give higher spending priority to the things emphasized in the incumbent president's party platform. It might indeed be that policy would have been the same regardless of the platform. But then the losers' and winners' platform emphases both ought to predict policy positively in the same way, which they do not.

How can that be explained? We agree with King and Laver that there are many causal forces driving policy. But the winners' platforms predict policy positively, the losers', often negatively; the opposite statements are generally not true. This returns us to our first point. We assert that the parties *in* government have a mandate and that the ones *out* do not. That is all we are saying. But we think that is quite a lot.

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Notes

King and Laver are very grateful to Neal Beck for his comments, Ian Budge and Richard I. Hofferbert for their data, Steven Voss for research assistance, and the National Science Foundation for (separate) research grants to King and Laver. The data and other information necessary to replicate this article are available from the Inter-University Consortium for Political and Social Research in a class 5 dataset listed under King and Laver.

1. Budge and Hofferbert describe this variable as being "lagged 2 years"; but from their more detailed description, it is apparent that they are using the term *lagged* in a nonstandard way. For an explanatory variable at year t, the same row in the data matrix has Y at time $t + 2$.

2. When comparing equation 1 with the equation originally published by Budge and Hofferbert, note that we have added conditional-expected-value notation and subscripts to the regression coefficients. We also use P and $(1 - P)$, instead of Budge and Hofferbert's separate notation for each.

3. To see this, note that we can rewrite equation 1 as $E(Y) = \beta_0 + \theta_r R + \theta_d D$, where $\theta_r = \beta_r + \beta_{pr} P$ is the effect of R and $\theta_d = \beta_d + \beta_{pd}(1 - P)$ is the effect of D. Budge and Hofferbert are interested in θ_r when the Republicans win ($P = 1$) and θ_d when the Democrats win ($P = 0$).

4. Using b with the appropriate subscript to refer to the least squares estimate of the corresponding β parameter, the variance of the sum $(b_r + b_{pr})$ is $V(b_r + b_{pr}) = V(b_r) + V(b_{pr}) + 2C(b_r, b_{pr})$. The first two terms on the right side of this equation are the squares of the standard errors implied by the asterisks in their tables, but the covariance in the last term cannot be derived from information in their article. Alternatively, one could regress Y on $R(1 - P)$, DP, RP, and $D(1 - P)$. The coefficients on these variables are then the effects of the Republican platform when the Republican's lose, the Democratic platform when they lose, the Republican platform when they win, and the Democratic platform when they win, respectively.

5. More specifically, they assume, incorrectly, that the disturbance term is composed of independent random variables with mean zero. In fact, both the independence and the mean-zero assumptions are probably false. Given the obvious time trends in both dependent and independent variables, the technique the authors use is unable to distinguish between the possibilities that the pattern described in their figure 1 is true (but sloping upward) and that it is systematically false (but sloping upward).

6. We present revised estimates using these lagged values of the dependent variable but note that many other time series specifications may also be plausible. If more observations become available, it might be possible to do more detailed analyses in order to be more confident of the specific time

series process in these data. For the present, we know that there is some time series process and the numerous studies of budget figures usually indicate low-order autoregressive processes, so that merely including a lagged value of the dependent variable will control for a good portion of this phenomenon.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN DECEMBER, 1993

The following articles, research notes and controversies have been scheduled for publication in a forthcoming issue of the *Review.*

David P. Baron. "Electoral Competition with Informed and Uninformed Voters."

Richard A. Brisbin, Jr. "Antonin Scalia, William Brennan, and the Politics of Expression: A Study of Legal Violence and Repression."

David Collier and James E. Mahon, Jr. "Conceptual 'Stretching' Revisited: Alternative Views of Categories in Comparative Analysis."

Walter Enders and Todd Sandler. "The Effectiveness of Anti–Terrorism Policies: Vector-Autoregression-Intervention Analysis."

Jeffrey C. Isaac. "Oases in the Desert: Hannah Arendt on Democratic Politics."

D. Roderick Kiewiet and Langche Zeng. "An Analysis of Congressional Career Decisions."

Arthur Lupia. "Shortcuts versus Encyclopedias: Information and Voting Behavior in California Insurance Reform Elections."

John P. McCormick. "Preparation and Response Regarding the Political Exception: Machiavelli's 'Accidents' and the Mixed Regime."

Walter R. Mebane, Jr. "Fiscal Constraints and Electoral Manipulation in American Social Welfare."

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Thomas M. Holbrook and Emily Van Dunk. "Electoral Competition in the American States." A Research Note.

Jon A. Krosnick and Laura A. Brannon. "The Impact of the Gulf War on the Ingredients of Presidential Evaluation: Multidimensional Effects of Political Involvement." A Research Note.

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POLITICAL THEORY

What's the Matter with Liberalism? By Ronald Beiner. Berkeley: University of California Press, 1992. 197p. $25.00.

True Tolerance: Liberalism and the Necessity of Judgment. By J. Budziszewski. New Brunswick: Transaction, 1992. 323p. $32.00.

These authors have contributed lively polemics against liberal moral theory. Both attack the commitment to neutrality in contemporary liberalism, which involves the refusal to take a stand on the question of the good life. Budziszewski sees his attack as coming from within liberalism, while Beiner's attack is based on the classical teleological tradition. They show a clear understanding that the purpose of liberal neutrality is to protect the liberal value of freedom. Beiner calls liberal neutrality a false theory, but what he really means is that liberal neutrality is a failed project. If liberal neutrality "does not discriminate between substantive consumer choices, it does privilege the consumer model itself, and this is a particular conception of human life and society that is deeply partisan and has been intensely contested, both in theory and in practice" (p. 8). In other words, the liberal attempt to be neutral about the good life fails. I had thought that the argument between liberalism and communitarianism was the most important dispute of our time. Beiner denies the reality of this dichotomy. If a communitarian order is one in which there is a shared understanding of the human good, then there is such a thing as liberal communitarianism. Beiner's argument is that "liberalism itself instantiates one particular vision of the good, namely, that choice in itself is the highest good" (p. 25). And he advises those who are committed to a substantive view of the good to avoid the communitarian label: "We can easily imagine a warm sense of community based on mutual participation in the decrepit ways of life opted for by our postulated autonomous individuals" (p. 28). But there is nothing intrinsically good about the experience of community, he says. From the point of view of substantive moral theory, it makes no sense to affirm community: "To affirm community as such is to abstain from judgment about the substantive attributes of a given community" (p. 31).

Liberals try to sustain a commitment to neutral political order by distinguishing between the right (rationally and politically authoritative principles of justice) and the good (ways of life freely chosen by individuals). The liberal state supposedly can maintain a neutral stance about individual choices of the good. Beiner agrees with those who say that the right/good distinction does not hold up, because the right is always derived from the good. This is because principles of right exist to sustain ways of life and are intellible only in light of the good or goods they serve. The liberal right sustains a way of life based on a contestable notion, the notion that choice is more important than any other value.

If the concept of right is not intellible apart from a concept of the good, then perhaps political theory should focus on the good and abandon the right/good distinction: "If it is our status as moral agents, or as agents who choose their own routes to happiness, then it is an empirical question whether we all do in fact form reflective life plans that warrant respect" (p. 82). The real question raised by rights claims is this: "On what grounds of what vision of human life, or in the light of what desired goods, should a society favor one set of rights over another?" (p. 93).

A philosophical examination of the question of the good life is always welcome. But Beiner's intends to influence political practice. His concern is "with the rhetoric of rights as a political phenomenon" (p. 95). Assuming that he does not wish to eliminate freedom of choice in the lives of persons but only to reduce its significance, he needs to consider the political implications of eliminating any "rights" claims that are not subordinate to a prevailing view of the good. In the absence of a politically, as well as philosophically, grounded teleology, society will be ruled by political victors who are purveyors of the prevailing view of the common good. Without a concept of rights distinguishable from that good, persons will have no basis for appeal against that good. Both communitarians and teleologists must deal explicitly with the coercive power of political institutions. If they reject the concept of rights, they must (each group in its own way) discuss safeguards against abuses of power and explain why those safeguards are conceptually distinct from rights.

The tone of Budziszewski's work is more polemical than Beiner's. But *True Tolerance* does contain a comprehensive critique of what can be called blanket moral neutrality. Through many arguments and examples, he demonstrates conclusively that thoroughgoing value neutrality is a self-defeating notion because making any choice is always on behalf of a value. In words addressed to Ronald Dworkin, he writes, "To demand that all be treated with equal concern and respect is to concede at the least the prima facia goodness of enjoying concern and respect, and perhaps the intrinsic goodness of according concern and respect" (p. 25). But if the value that liberals support is freedom of choice, then liberal neutrality should be understood as a secondary principle that serves the primary value of freedom. The political commitment to be neutral about a particular dispute (the good life) for a reason (to protect freedom) is neither self-contradictory nor unintellible.

The positive contribution Budziszewski wishes to make is to our understanding of the place of the virtue of tolerance in political and moral theory. Like Beiner, he disassociates himself from any form of communitarianism that endorses the value of community regardless of the moral content of communal values (p. 5). Tolerance is not moral neutrality; it is a moral virtue. What, then, is tolerance? To tolerate something is to put up with it. But Budziszewski insists that "true tolerance is the art of knowing when and how to tolerate" (p. 7). True tolerance is possible when we know which evils we must put up with, which depends upon knowing which goods we must preserve (p. 7). The basic difference between liberals and their communitarian or teleological rivals is over which "evils" we must put up with to preserve the good of freedom, to which we all share a commitment. If we are to remain free, we must, according to liberals, put up with differing views about the good life and about the philosophical standing of that subject. Communitarians and teleologists continue to argue that society must adopt an authoritative view of the human good while denying that their view menaces freedom.

University of New Orleans LYLE A. DOWNING

Politics, Gender, and Genre: The Political Thought of Christine de Pizan. Edited by Margaret Brabant. Boulder: Westview, 1992. 240p. $39.95.

Democracy without Women: Feminism and the Rise of Liberal Individualism in France. By Christine Fauré. Bloomington: Indiana University Press, 1991. 196p. $29.95.

Each of these works contributes, in distinctive and related ways, to the project of discerning and constructing lineages of modern feminism. Although the term *feminism* did not appear until 1892, its relatively recent linguistic coinage does not automatically disqualify this project as an anachronistic effort to read feminist desires, sensibilities, motifs, and strategies into premodern eras. Such a project, however, is simultaneously facilitated and hampered by two interrelated problems, the first of which is astutely described by Christine Fauré:

> Suppose you want to come to grips with recent changes in the status of women in contemporary society, and you feel the need for a personalized approach to questions normally addressed by the objective rules of historical method. This undertaking will be all the easier for you in that there exists absolutely no tradition on the subject. Up to the present, feminism and its history have held a small place indeed in the study of political philosophies. (p. 1)

The second problem confronting the genealogist of feminism is that history and political theory have so cluttered the discursive landscapes of political history with so many conceptual markers and narratives of "significance" that have assiduously ignored women that it is now difficult to find the women at all. Those who doubt this are encouraged to consider Eric Hicks's suggestion, proffered in the opening essay of *Politics, Gender, and Genre,* that "the significance of Christine de Pizan as a political writer lies outside the *history* of ideas" because there was no academic tradition capable of recognizing this medieval poet and political author, who wrote in the French vernacular, rather than Latin, and who was a woman (p. 8). Because "the habit has never really been formed of entering Christine's name in the canon of political authors," we will not find her there (ibid.). Part of what is at stake, then, in contemporary reassessments of Christine, is the figure of "the political author."

Christine de Pizan (1364–1403?) figures prominently in both of these works under review. In *Politics, Gender, and Genre,* she is the centerpiece of 14 essays exploring the substance and method of her work as a political author. *Democracy without Women* begins with a chapter on Christine. Fauré's starting point for her historical investigation of feminism in France is deliberately provocative and innovative, an instructive example of how efforts to delineate the lineages of modern feminism must break ranks with established narratives and chronologies of political history. Fauré's heretical move involves her deliberate displacement of the French Revolution as the logical origin of feminism in France. This is related to her explicitly stated effort to challenge "the hegemony of a 'spirit of socialism' which reigned over the analysis of social transformations" (p. 14). Socialist and communist movements, she asserts, have done little to advance the cause of women, because they privilege the family. Women's participation in the French Revolution "has meaning only in the perspective of the philosophical, religious, and political 'revolutions' that preceded it" (p. 15). These revolutions, according to Fauré, involve notions of equality, liberty, and individual human rights. Lineages of modern feminism, then, must be linked up with lineages of liberalism. Both terms (*liberalism* and *feminism*) are treated as philosophical and political currents that predated their nearly simultaneous linguistic appearance in the last decade of the nineteenth century. By *feminism* Fauré means efforts to "redress the balance of social and political power between men and women" (p. 9). Her project is to "reveal a solid logic" informing "those texts that affirm the natural and legal equality between the sexes" (p. 16). Her quarrel with political theory, intellectual history, and the history of ideas is that these texts are rarely treated as "significant" works.

Readers of Fauré's newly translated work will be treated to a cornucopia of authors and texts—some heretofore obscure, others recognizable. Her narrative structure, defamiliarizing established political authors such as Burke, Montesquieu, Rousseau, and Condorcet, brings such outlying figures as Christine de Pizan and Mary Wollstonecraft into the conversation and introduces English-speaking audiences to relative unknowns such as Marie de Gournay (Montaigne's adopted daughter, author of *De l'égalité des hommes et des femmes* [1622] and *Grief des dames* [1626] and those women who gained notoriety, but no enduring rewards ("disincentives" would be more apt), for their participation in the French Revolution.

For this reader, Fauré's work is simultaneously challenging and vexing. It is not always easy to discern her narrative of feminism as an organizing structure for the detailed, yet selective, presentation of events, texts, actors, and authors ranging from the fourteenth to the late twentieth century in less than two hundred pages of text. The Anglo–feminist reader, particularly if he or she is sympathetic to Carole Pateman's critical work on the liberal tradition of contract theory, will have a difficult time appreciating the revolutionary and feminist implications of liberal theory. To the extent that liberal discourse constitutes the fabric of U.S. political culture and accounts for that culture's resistance to women's empowerment, liberalism has often seemed to be part of the problem. Yet Fauré's understanding of liberalism is rather different from ours in at least one crucial respect: it does not require the infamous public–private distinction. In a similar vein, Susan Okin's interrogation and recuperation of liberal theory also confronts and contests how the category of the "private," which subsumes women as individuals within the unit of the family, has actively repressed feminist implementations of liberal core values. One of the benefits for Anglo readers of Fauré's work is that it offers a fresh perspective on liberalism-as-feminism.

What can it mean to assert, as Fauré does, that Christine de Pizan stands at the origin of modern feminism? In what sense other than the flagrantly anachronistic was Christine de Pizan a "feminist"? This question frames a number of the essays in *Politics,* *Gender, and Genre*. The varieties of response to this question cluster within three basic camps: one treats the question itself as inappropriate, arguing that Christine's "significance" as a political author has nothing whatsoever to do with postmedieval notions of feminism; one denounces Christine as an antifeminist; and a third appraises her appreciatively as an exemplar of, and spokeswoman for, contemporary Western feminist values.

None of the chapters in this collection explicitly endorses the first position, although it is discussed by Eric Hicks in "The Political Significance of Christine de Pizan." The modern feminist case against Christine is developed by Sheila Delany, in "History, Politics, and Christine Studies." According to Delany, Christine was "a reactionary and a prude" who defended conservative and patriarchal political arrangements (p. 197). Her "attempt to silence criticism of a terminally corrupt ruling elite" was nothing more than "self-interested sycophancy" (p. 199). Christine M. Reno's "Christine de Pizan: "At Best a Contradictory Figure"?" takes issue with Delany's characterization of Christine as a political author corrupted by the system of patronage within which she wrote. According to Reno, Christine opposed the human suffering entailed by social and political upheaval, rather than revolution itself; was far more critical of the nobility than she is given credit for; and displayed "a consciousness of female solidarity . . . that predates by almost six hundred years the modern construct of 'sisterhood'" (p. 183). In another feminist vindication of Christine, Margaret Brabant and Michael Brint explore her complex rendering of the dialectics of identity and "alterity," which are also operative within contemporary feminist inquiry. For Brabant and Brint, Christine pursued an instructive path between universal humanism and respect for difference.

In other thoughtful essays, Christine is interrogated, but not vilified, for her contributions to an emergent French nationalism and her complicity with anti-Judaic sentiments. Other essays explore the writerly bravura of an author who had mastered the literary genres of her era and who deployed them in remarkably innovative and politically transgressive ways. Several essays address the fascinating subject—simultaneously literary and political—of female authority and authorial voice.

Christine emerges from these essays as a complex, exemplary figure who defies simple categorization. This assessment is also advanced by Fauré, who suggests that Christine's Augustinian feminism is "conservative" by contemporary standards, and that her position in the debate over *Le Roman de la Rose* is "essentially at odds with any progressive definition of feminism" (p. 23). Why, then, does Fauré locate Christine within a privileged, yet admittedly paradoxical, point of origin in her lineage of modern feminism? Her answer is that Christine's significance lies in her negotiated mastery of a complex, dangerous, misogynist environment: it is by virtue of her *independence* as a woman and as an author that she is significant—which is to say that lineages of modern feminism lie beyond the conventional, comfortable markers of "progressive" versus "liberal." The implications of this claim for contemporary Western feminism are well worth pondering.

University of Washington CHRISTINE DI STEFANO

Secession: The Morality of Political Divorce from Fort Sumter to Lithuania and Quebec. By Allen Buchanan. Boulder: Westview, 1991. 174p. $38.50 cloth, $14.95 paper.

Secession addresses a problem that, Buchanan thinks, cuts to the heart of liberalism's universalism and its exclusive focus on individual interests and rights (p. 8). If a group can better secure its interests (broadly conceived) by secession from a polity than by remaining in it, then it seems that the members of the group are better served by focus on the group. On this account, secession is merely a political manifestation of underlying communitarian commitments. Buchanan wishes to give such commitments their due by "significantly modifying liberalism [and] pruning back its excessively individualistic elements" (p. xv).

Buchanan combines positive, moral, and what one might call positive constitutional analysis with his issues. The positive, explanatory discussions of the nature of various ethnic and national conflicts is good and enlightening—the variety and intensity of the cases is distressing and their varied complexities are fascinating. The positive constitutional analysis builds on the assumption that people (or peoples) just do have various values and that any arrangement that vaguely serves the mutual advantage (p. 43) or balances the rights of groups within some borders or potential borders has some claim on their national organization. That analysis is finally worked out in arguments for including a clearly defined right of secession in constitutions (chap. 4). At the time of Buchanan's writing, only the Soviet Constitution provided a right of secession (p. 9)—that constitution is now defunct after total secession.

The moral analysis is much less coherent. Buchanan does a strangely atheoretical theory of a kind that is common in moral philosophy but that has no reputable analog in positive theorizing. There are many moral theories, which are grounded in dramatically different assumptions. One might suppose that the analysis of a given problem, such as secession, would be carried on within one of these theories by starting from its assumptions. For example, one might focus on some substantive good, as in utilitarianism; on a priori principles for action per se, as in rationalist Kantianism; on the virtue and character of the agent, as in Aristotelianism and much of contemporary communitarianism; or on procedural constraints, as in libertarian and some other rights theories. Instead, Buchanan recurs to all of these. His conclusions can therefore seem ad hoc and idiosyncratic. Another theorist working the same vineyard in the same manner might reach different conclusions. In joining these elements, Buchanan often must have recourse to his intuitions. Sometimes, these intuitions are about how, for example, rights trade off against welfare considerations. Sometimes, they seem rather to be that oddity of moral theory, direct intuitions about the rightness or wrongness of settling his issue one way or another. If we know such things, the theory is otiose window dressing and rationalization. Perhaps sometimes we know such things (as skeptics note, it is unfortunate that even when we think we know them, we do not agree about them), but it is peculiar to suppose we intuitively know them for complex problems such as those of political statehood and the morality of secession.

Buchanan is fully aware of his eclecticism in moral theory. Indeed, he expresses "astonishment" that anyone could be bothered by it, saying most philosophers agree that no single principle "is a plausible basis for all moral decisions" (p. xiii). Perhaps the class of all philosophers is a moral mess. Members of the smaller class of moral and political philosophers commonly assert the exclusive rightness of one principle or another. Buchanan's is a bold methodological move. He supposes that traditions of thought and practice merit central place in analyzing practical problems. In effect, he supposes that because we have had a tradition, it is therefore compelling, even right or at least partly right.

The kind of moral theory that plays the strongest role in Buchanan's own arguments is rights theory, as in the bulk of his discussion of "the morality of secession" (chap. 2). He contrasts a welfarist efficiency principle with "moral considerations," as though welfare is subordinate to rights (p. 48). Apart from religious ethics, rights theory is perhaps the nearest thing we have to a vernacular moral position or tradition, at least in the West. And there's the rub: rights theory has not been a central tradition in much of the world, especially in much of the world now torn by ethnic conflicts and urges for secession and suppression. Rights theory is strongest where the market prevails. Many ethnic groups around the world are unlikely to be moved by claims of the rights of other groups. Their views are more nearly the group-level analog of solipsism: what they want is therefore what they think is right, and this is not a matter of derivation from Buchanan's essentially universalistic group rights.

In part because he has taken a strong rights view, Buchanan's book is likely to be an influential contribution to its issue. It defines one of the standard positions and gives proponents of other positions a relatively clear target. Its careful review of arguments for and against secession will make the work of others much easier.

New York University Russell Hardin

A Communitarian Defense of Liberalism: Emile Durkheim and Contemporary Social Theory. By Mark S. Cladis. Stanford: Stanford University Press, 1993. 339p. $39.50.

The theoretical strategy of this book is quite straightforward. Mark Cladis sees both sets of protagonists in the liberal–communitarian debate as addressing valuable and ineliminable aspects of our social situation, and he hopes to delimit a broader theoretical standpoint capable of encompassing what both sides in the debate have to offer. Cladis proposes Emile Durkheim as the exemplar of this more encompassing synthetic position, surpassing what Amy Gutmann calls "the tyranny of dualisms" (p. 2). What results is a highly eclectic and fairly agreeable survey of the contemporary scene in political philosophy.

It does not really come as a surprise that various nineteenth- and early-twentieth-century liberals do not readily fit into an individualism–communitarianism dichotomy; one thinks immediately of Tocqueville, J. S. Mill, Green, Hobhouse, and John Dewey. It is worthwhile to learn that Durkheim belongs in this company of communitarian liberals, since he is commonly thought of as a conservative sociologist. Indeed, it was precisely in order to display Durkheim's liberalism that Steven Lukes made available for an English-speaking audience the *Dreyfusiste* polemic "Individualism and the Intellectuals" (*Political Studies* 17, no. 1 [1969]: 14–30). The example of Durkheim shows, if it needs showing, that the liberal–communitarian dualism made so familiar to us by contemporary social theory can, in principle, be straddled, since one can be as concerned about anomie in modern societies as Alasdair MacIntyre, and as anxious to relate individuals to a common *Sittlichkeit* as Charles Taylor and nonetheless be as liberal in one's politics as John Rawls or Ronald Dworkin.

Cladis basically recasts Durkheim as a Rortyan pragmatist: on his account, Durkheim offers us liberal social criticism "without resorting to foundationalist or universalist arguments" (p. 27). Yet it seems odd (to say the least) that one would exert oneself to defend Dreyfus against the more unsavory aspects of French tradition without invoking universal principles. The vehicle for Durkheim's political activism was, after all, called "Ligue pour la Défense des Droits de l'Homme," which certainly sounds neither nonfoundationalist nor nonuniversalist. Cladis himself points out that social theory understood as a normative discipline is needed to sift through the traditions and historical practices by which we are shaped, in order to discriminate "what is to be preserved and what is to be reformed" (p. 28). But if social life is contingency through and through, resting on no transcultural foundation, by what standard can we possibly distinguish worthy traditions and conventions from those in need of reform? Cladis views Durkheim's historicism as a theoretical advantage, without appreciating how it might vitiate the critical bite of Durkheim's normative enterprise.

Durkheim's thought, as Cladis presents it, is attractively contemporary; he even goes so far as to label Durkheim "a precursor to much sensible, postmodern thought" (p. 250). To be sure, it is instructive to learn that Durkheim was more of a liberal and less of a communitarian than many have thought. But Cladis's own preferred theoretical option, *internal social criticism,* is not original; nor is it terribly incisive. Like Rorty, he sets up foundationalism as a straw man. To cite one of Cladis's formulations, it is the attempt "to escape history in order to discover an ahistorical overarching structure to the moral universe" (p. 238). Admittedly, if the alternative to internal social criticism (the anchoring of theory in historical practices and narrative traditions of given societies) must involve this radical a break with history in general, we would be better off remaining tied "to the cave" (p. 316, n. 3). But which theorist that we can think of counts as a foundationalist in this sense? This implausible characterization of an alternative to internal social criticism allows Cladis to be fairly evasive in offering a definite ground for social criticism. He tells us repeatedly that standards of judgment are available while avoiding any clear statement of exactly what entitles us, given the truth of historicism, to uphold such standards of judgment.

Cladis would say, à la Walzer or Rorty, that Durkheim's commitment to liberal rights of the individual arises from within a particular moral tradition, rather than implying an external standard that is held up against the political community in order to judge the moral adequacy of the latter. His situating liberal rights within the historical practices of an established moral community is what renders Durkheim both liberal and communitarian (as could be said for Walzer and Rorty, as well). But if this is so, liberals are deprived of any moral standard by which to measure or criticize illiberal or antiliberal societies that occupy themselves with the pursuit of a different set of intimations. This historicist liberalism seems very ill equipped to say why a liberal moral vision should commend itself to moral communities that are not already committed to liberalism. Moreover, since Durkheim was fully aware that French liberalism at the time of the Dreyfus affair remained highly precarious and far from culturally entrenched, it is not at all clear why the appeal to the ongoing liberal tradition internal to French practice ought to suffice in defending and justifying liberalism. What Cladis has learned from the communitarian challenge to liberalism appears to have lightened, rather than increased the theoretical burden to be assumed by liberals.

University of Toronto RONALD BEINER

Self/Power/Other: Political Theory and Dialogical Ethics. By Romand Coles. Ithaca: Cornell University Press, 1992. 203p. $29.95.

This is an engaging book that intricately weaves interpretations of Augustine, Foucault, and Merleau-Ponty together in order to argue for an agonistic, dialogical politics of difference. Coles's stated assumption is that our differences are fertile ground. Neither abstract individualism nor communitarianism takes advantage of these differences. When "dialogically engaged," these differences, or "edges," provide us with the resources we need to contest domination and assert our freedom creatively (p. 6). Aside from opening and closing references to ecology and a couple of passing references to the freezing out of "women's cacophonous being," Coles does not defend this assumption about the "pregnancy of edges" (p. 139). Nor does he defend his critical assessment of the modern culture of productivity within which a new ethos of dialogue and difference is to be brought to bear. If we accept these assumptions, Coles implies, then we will want to explore our differences more creatively, more cooperatively, and in much more depth than have contemporary political theorists of almost all stripes.

Depth is the key word here, and this is why Augustine is important. Coles clearly presents Augustine's account of the pagan self and the confessing self in order to develop a conception of depth, which he will build upon in later chapters. Augustine rejects pagan "ontological conceit" and constructs, in its place, a humbler—but compulsive—self, whose sole object is reflection on its own history. Through confession, the self can figuratively clean house. Lustful desires can be routed, and a new self can be formed: "Remembering *as such* is a focusing, healing, strengthening, and disciplining activity in Augustine's view" (p. 37). As the self goes deeper into its memory, it gradually sheds its ontological conceit. With greater depth, comes greater freedom from the desire for power and lust. The deeper you go, the freer you become; "for only this self is sufficiently unified and self-concious to carry out the work of freedom" (p. 47).

There are two political dimensions to this conception of the self: one good, one bad. First, Augustine's project is political, according to Coles, in the sense that confession is a public act. The confessing self confesses among others, and this constitutes a "politics of exemplarity" (p. 50). On the other hand, confession leads to a kind of flattening out and intolerance of difference. As Coles says later, the confessing self suffers from an ontological conceit of its own: now God, not man, is the one source of Truth. Foucault and Merleau-Ponty, Coles argues, both carry Augustinian themes forward in one way or another. Even though he finds much to value in Foucault's critique of depth, he argues that Foucault neglects the resources for dialogue that exist along the lines that divide us. And while Merleau-Ponty presents a richer, more complex account of the self than Foucault, he also ultimately fails to translate his ontological and epistemological views into a coherent practical ethic.

For Foucault, the institutionalized practice of confession constructs a self that is alienated from the other and stripped of its freedom. Coles argues that Foucault's treatment of the modern episteme in *The Order of Things* (1973) should be read in conjunction with his genealogical studies, because "modern thought in its very being manifests a type of power" (p. 71). Abstract schools of thought (not just prisons and clinics and local knowledge) have shaped the relationship between self and other. Augustine (not just Bentham) got it right—too right.

Does Foucault have anything to offer to those who want to contest the dual triumph of normalization and the classical episteme? No, not if they are looking for detailed institutional strategies. But he does offer a powerful theoretical attitude toward difference that might be able to break the normalized bond between self and other. It is the attitude of Baudelaire. In Foucault's words, "We have to create ourselves as a work of art" (p. 81). This is not an idiosyncratic or solipsistic enterprise. The project is to give up the pipe dream of a unified self and learn to view ourselves as works of art constructed as we dwell dialogically in the light of each other's differences (p. 89). Furthermore, Coles discerns a theory of "belonging" in Foucault's notion of an "ironic heroization" of the present. This seems to be a stretch, as Coles acknowledges; but leaving this aside, Coles's reading of Foucault is plausible and tailored to fit his own purposes.

Merleau-Ponty requires more work. While Foucault's account of the social construction of the self and the eradication of difference is embedded in an explicit theory of power as normalization, Merleau-Ponty's political views are not easily connected to his metaphysics and epistemology. Coles's strategy is to keep depth at the center of his interpretation, which makes good sense. While Foucault had attacked the Augustinian conception of a deeper self, showing how it was a product of power relations, Merleau-Ponty breathes new life into the idea of depth: "Being with different others who transcend our grasp pulls us out of anonymity and hurls us into an intercorporeal world with depth and distinction" (p. 132). Sensitive to the depth of our being when it is shaped through dialogues with others, Merleau-Ponty is also drawn to the artist who, like Cezanne, works within the contingencies of life to give it meaning in an exemplary way (p. 147). The move from this ideal to Merleau-Ponty's views on parliamentary democracy is somewhat abrupt, but one must admire how Cole sets up the problem.

In the final chapter, Coles cuts adroitly back and forth over his three subjects, reemphasizing and elaborating the differences and similarities that he has introduced along the way. Like Charles Taylor and William Connolly, Coles has used Augustine to anticipate some of the modern problems of Cartesian selves. In addition to inwardness, however, he also discovers in Augustine a sense of tragedy worth restoring. And against Foucault's criticism of Augustinianism, he finds Foucault's treatment of violence too sweeping and indiscriminate. Merleau-Ponty's rhetoric and ontology, he suggests, are more suited to a politics of artistic dialogue than Foucault's. These closing pages on violence and other forms of domination are not argued in great detail. The mediating middle term in Coles's title remains undertheorized. In this book, Coles settles for warning us from the excesses and overstatements in Foucault and alerting us to the possible political uses of Merleau-Ponty's revaluation of depth. Given the creative readings that form the body of the book, one looks forward to more.

Michigan State University STEPHEN L. ESQUITH

Machiavelli in Hell. By Sebastian de Grazia. Princeton: Princeton University Press, 1989. 497p. $55.00 cloth, $14.95 paper.

The Machiavellian Cosmos. By Anthony J. Parel. New Haven: Yale University Press, 1992. 203p. $30.00.

Here are two books on Machiavelli of unusual distinction, composed by authors who have spent a good portion of their lives in loving study of a great man whom they need to understand and so do not wish to deconstruct. The wisdom resulting from their devotion reminds one of Machiavelli's recommendation that the hunter become a knower of sites by knowing one site well, for they show just how much political science can be learned from one solitary source if only it is chosen and examined well. Their scholarship might be thought narrow by political scientists, derivative and submissive by political theorists; but in a competition before impartial judges for who teaches the most about politics, these two would quiet such critics and easily vindicate their choice of subject and method. Yet their books are as different as plot and theme.

Sebastian de Grazia has done the trick that is the dream of all of us in the profession, whether we admit it or not: he has produced a serious book for the general reader. His intellectual biography of Machiavelli (which won and deserved a Pulitzer prize) shines with artistry of design, beautiful writing, and the kind of originality that always comes from accuracy and thoroughness. De Grazia has the whole of Machiavelli in his mind, not just the major works, and to every disputed point of interpretation, he brings all the relevant passages, not just those which make him seem wise. He quotes liberally in his own translation, which is far above the usual standard, so that the reader can follow attentively what the author makes of Machiavelli's words. Although he does not cite secondary literature, he has absorbed it and used it. He has brought together what little is known of Machiavelli's life and the feats of his mind (also little known) without slighting the latter—in this regard surpassing Roberto Ridolfi's well-known biography.

What is Machiavelli's greatness according to De Grazia? (For he calls him Niccolò—and the Italians call Signor Alighieri *Dante*—in recognition of his greatness, not out of New Jersey informality.) Niccolò had a "great liberating design" (p. 216) energizing men for politics; and for that design, the great problem is to reconcile the dubious deeds needed for liberation with the common good that is the end of politics. De Grazia is too sophisticated to accept Machiavelli's evil reputation as the truth and too honest to dismiss it. But he cannot quite make himself believe that *from evil comes good;* so for him, the good result reacts on the evil cause and excuses it as merely the appearance of evil. De Grazia gives us a Machiavellian excuse as Machiavelli's intent. I pass that judgment reluctantly, because the virtues of De Grazia's analysis include such excellent formulation as the "un-golden rule," virtue "at first blush," and the "rhetoric of imposture" (pp. 296, 299, 309). But in his book, the one missing theme of Machiavelli's is *acquisition*. Acquisition is the end of one's country, as of oneself; and when Machiavelli said he loved his native country more than his own soul, he did not specify that his country had to be good. In other words, its good was not more than survival through expansion; anything more (including all the beautiful works of his time except for his own books) are politically corrupting consequences of leisure, which De Grazia discusses well, but not well enough.

Machiavelli in hell is not in a fearful place; he is where he would be put (according to a story about Machiavelli that De Grazia accepts as from Machiavelli) by a benevolent Deity disinclined to punish benefactors of mankind. There, Machiavelli would find himself in a "goodly company" of conversation partners (chap. 14). In that unlikely event, would not Machiavelli attempt, contrary to De Grazia's expectation, to dominate the conversation? For if Machiavelli's truth is "effectual truth" (*The Prince*, ch. 15), it is political truth—the truth of deeds, not words, of results, not precepts—the truth of domination. Reading Machiavelli is not entering a conversation among equals; it is a frightening discovery that one's reason may lead one into tyrannical subjection.

Everyone knows that Machiavelli on occasion personified Fortune so as to install her as a deity governing human affairs; and some are aware that he spoke of two "humors" in every state, the popular and the princely. But Anthony J. Parel, in a book with an original thesis, connects Machiavelli's astrology and medicine to raise a doubt whether, with such premodern doctrines underlying his politics, he could have been the founder of modern political science or of modernity as a whole. This is the first book, I believe, devoted to the nonpolitical in the most political thinker that ever was.

To make his point, Parel provides valuable information on the astrological debate in Florence at the time and on the Renaissance science of medicine; and he searches through Machiavelli's works and in such central notions as virtue, religion, corruption, and liberty to bring out the nonpolitical "underpinnings" (p. 156). The underpinnings of politics are also limitations on politics; for Machiavelli's princes cannot control the chance of human affairs or institute irreversible progress if heaven, fortune, and human temperament stand in the way. In contrast to De Grazia, Parel considers Machiavelli to be anti-Christian, the first philosopher to offer "a justification for culpable evil and injustice" (p. 160). But he was constrained by his premodern premises, which were pagan, if not Aristotelian. Now that the premises have collapsed, Parel would like to have Machiavelli's political and ethical conclusions abandoned. But to see that welcome event, we must first be sure that Machiavelli's argument is *premised* on the nonpolitical and not merely *indebted* to it as Parel also says (pp. 138, 161). In the latter case, one could suggest that Machiavelli argues for politics as "the highest human calling" on political grounds and merely brings to his aid premodern doctrines of astrology and medicine (p. 154). These are more enabling than constraining—more progressive than conservative—because astrology provides a provisional alternative hostile to Christianity (as we see from the attacks on astrology in Machiavelli's time by Pico della Mirandola and Savonarola, which are discussed well by Parel), while the theory of humors substitutes for Aristotle's use of political opinion and ushers in the modern theory of the passions to be found in Descartes, Spinoza, and Hobbes.

Both authors operate with the sincerity premise that is conventional to modern scholarship. For whatever reason, Machiavelli is sincere and never tries to trick his readers. De Grazia quotes Machiavelli's statement in a letter, "I never say what I believe, nor believe what I say, and even if sometimes the truth is told me, I hide it among so many lies that it is difficult to find it again" but makes nothing of it (p. 364). Parel accepts that Machiavelli uses "the orthodox Christian position for rhetorical effect" but asserts that he sincerely believed in the divinity of Lady Fortune (p. 82). De Grazia says that although Machiavelli knows how to tell a joke when he announces one, in all his political works, major and minor, "not a smile cracks his face" (p. 335). Nothing would have amused our Niccolò more than this remark.

Harvard University HARVEY C. MANSFIELD

Environmentalism and Political Theory: Toward an Ecocentric Approach. By Robyn Eckersley. Albany: State University of New York Press, 1992. 274p. $44.50 cloth, $14.95 paper.

Most political theory still proceeds as if the human world floats free from nature. Yet arguably, environmental limits now pose the most fundamental question for both politics and political theory; and in neither realm have convincing answers been found. Robyn Eckersley seeks an answer and effectively dispenses with some dead ends without necessarily taking the reader all the way to the promised land. But that would be too much to ask of one book, and Eckersley's explorations are none the less instructive for the imprecision of their end point. With this book, she establishes herself as a leading Green political theorist.

The first three chapters are devoted to the establishment of an ecocentric standpoint, which Eckersley believes is the only adequate evaluative response to environmental crisis. Ecocentrism, for Eckersley, involves the emancipation of people *and* nature. Thus, it recognizes the diversity of human interests in the natural world, as well as the interests of nature itself, and so avoids the misanthropy found among, for example, some deep ecologists. She suggests that the two litmus issues for ecocentrism are human population reduction and wilderness preservation for its own sake, rather than for human interests (p. 29). However, the population litmus test might exclude ecofeminism, which she gives as one of her three examples of ecocentrism (the other two being intrinsic value theory and transpersonal, or deep, ecology); for many ecofeminists oppose coercive population control. And wilderness preservation really only makes sense as a key issue in societies that have wildernesses to preserve, such as Eckersley's own Australia. Wilderness has played a far smaller role in Green politics and political thought in Europe.

Having established this central principle, Eckersley devotes the remainder of the book to an ecocentric scrutiny of four traditions of radical political thought: *Marxism, critical theory, ecosocialism,* and *ecoanarchism.* Liberalism, conservatism, and authoritarianism receive only summary dismissal; and free-market environmentalism is not mentioned. Eckersley's selections here make sense as one trajectory in the quest for an adequate ecocentric theory, though she dismisses Marxism as hopelessly cornucopian and anthropocentric. Her justification for attending to Marxism is that it provides a foil for her subsequent discussion of more promising doctrines. Moving to critical theory, she concludes that the hints of ecocentrism once found in Adorno, Horkheimer, and Marcuse have been thoroughly purged by Habermas. Habermas echoes more orthodox Marxists in considering that the only fruitful human attitude toward the natural world is one of mastery and manipulation. Here, Eckersley might have considered the possibility of rescuing critical theory from Habermas; but instead, she dumps it altogether. Ecosocialism, for its part, is found wanting because it sees in ecological crisis only a means to the end of overthrowing capitalism and creating a better society defined in anthropocentric terms.

This elimination means that of the four political theories considered, only ecoanarchism (identified with Murray Bookchin) passes the ecocentric test. Yet Eckersley is not an econanarchist. For dispensing with the state and transferring authority to local communities will produce decidely antiecological results in places like Utah, where current levels of wilderness protection are entirely due to the efforts of a distant national government. Moreover, Eckersley believes that multiple and interacting levels of ecosystem organization should be matched by multiple and interacting levels of government. Thus, her preference is for "a multi-tiered, democratic decision making framework" including a state, the rule of law, basic human rights, and checks and balances (p. 178). She considers it crucial that this framework be matched by the cultivation of an "ecocentric culture" (p. 185). Now, this institutional framework sounds suspiciously like what we already have, at least in federal liberal democracies. If so, the only truly novel component of Eckersley's prescriptive theory is ecocentric culture—in which case, the changes she proposes are really at the level of morality, rather than politics, so that her quest for an ecocentric political theory ends where it began, with a statement of the need for an ecocentric perspective.

Eckersley is far from alone in concluding with a plea of this sort, but any disappointment in the ending should not detract from a recognition of the power and intelligence of her earlier analyses and critiques. *Environmentalism and Political Theory* shows that political theory really can engage important real-world issues, concerns, and actors in comprehensible and insightful fashion. Indeed, the book can also be read as a contribution to the ecocentric side of debates that engage Green activists worldwide. Green political theory is alive and well and living in Australia.

University of Oregon JOHN S. DRYZEK

Forbidden Grounds: The Case Against Employment Discrimination Laws. By Richard A. Epstein. Cambridge: Harvard University Press, 1992. 530p. $39.95.

The Constitutional Logic of Affirmative Action. By Ronald J. Fiscus. Edited by Stephen L. Wasby. Durham: Duke University Press, 1992. 150p. $17.95.

Affirmative Action and Justice: A Philosophical and Constitutional Inquiry. By Michael Rosenfeld. New Haven: Yale University Press, 1991. 373p. $30.00.

The Civil Rights Act of 1964 and related legal pronouncements enacted a basic principle into public law which enjoyed wide public support then and now: that it is wrong and illegal to engage in "first order discrimination" in certain contexts important to our public and professional lives. First order discrimination is discrimination against individuals "on the grounds that they are inferior or different" simply because of their race or gender (Rosenfeld, p. 4). Such discrimination violates what Rosenfeld calls the "postulate of equality," which holds that "all individuals are equal qua individuals" (p. 20). The constitutional doctrine used to enforce this postulate is the "antidiscrimination principle." The antidiscrimination principle supports equality of opportunity by prohibiting intentional invidious discrimination. As Epstein shows, the principle proscribes "unequal treatment," not unequal results or impacts (ch. 10).

The simple antidiscrimination principle is consistent with the standards of liberal constitutionalism, which traditionally favor or emphasize: equal treatment and opportunity over equal results; individual rights over group-oriented rights; redressing inequality caused by discrete, identifiable acts of discrimination, rather than that caused by less identifiable societal discrimination (Rosenfeld shows that this difference leads to the distinction between "atomistic" and "ecological" perspectives on discrimination and affirmative action [pp. 211–13]); compensatory justice over distributive justice; and redressing inequality without violating or harming the equal treatment rights of innocent third parties who have not themselves been guilty of first order discrimination.

Unfortunately, the antidiscrimination principle has not led to the equality of condition that many of its supporters had envisioned. Eradicating the effects of historical first order discrimination has proved annoyingly difficult, especially in matters of race. Accordingly, the courts have allowed or even ordered quota-oriented affirmative action remedies in several contexts, including school attendance patterns, employment policies, and the dispensation of federal contracts. Yet the courts have vacillated because of the fear of abandoning liberal principles which significant numbers of Americans accept. Legal policy is torn between the poles of equal treatment and equal result or impact. Sisyphus simply cannot get the stone up the mountain. As usually happens when such indecision reigns, many thinkers are unhappy with the status quo.

The three books under review reveal the depths of this discontent. Rosenfeld and Fiscus focus on affirmative action and the Fourteenth Amendment, while Epstein discusses the effects of the civil rights statutes (Title II on public accomodations, Title VII on employment, and the new Disabilities Act). Each of these very different writers acknowledges and condemns the legacy of racism in America, and each criticizes the antidiscrimination principle. Yet Rosenfeld and Fiscus wish to resolve the equality conundrum by cutting the Gordian knot of bad faith and embracing a full blown policy of affirmative action. Their model in constitutional law is the ecological approach taken by the dissenters in *Regents of University of California v. Bakke* (1978) and *City of Richmond v. J.A. Croson Co.* (1989), who would have authorized quota-oriented affirmative action plans to bring about, respectively, more racial balance in admissions to graduate/professional school and the allocation of government construction contracts. They ask Sisyphus to go up an easier mountain. Epstein advocates an antipodal approach which would rescind most of the edifice of antidiscrimination law in favor of a libertarian policy based on the freedom of contract. He tells Sisyphus to simply go around the mountain.

Rosenfeld's dense yet powerful book presents a complex argument that leads to a defense of the dissenters' positions in *Bakke* and *Richmond*. He argues for a strong form of compensatory affirmative action which goes beyond the antidiscrimination principle: "[A]ffirmative action should be permissable under certain circumstances even in the absence of invidious past first-order discrimination. These circumstances are present when the prospects of success of the members of a particular group have been reduced relative to those of the remaining members of society as the result of socially induced deprivations that may not have been unjust when they took place" (p. 328).

Rosenfeld is aware that this recommendation conflicts with prevailing liberal theories of justice, but he deems conventional approaches unworthy of the task. Thus he embraces a new form of social contract theory that goes beyond these models of justice: he discusses and rejects libertarian (Nozick's *Anarachy, State, and Utopia*, 1974), contractarian (Rawls' *A Theory of Justice*, 1972), egalitarian (Nagel's *Mortal Questions*, 1979), and utilitarian models of justice (chs. 2–5). He offers his own dressed up version of Rawls, in which individuals who assemble to formulate social policy are not ignorant of their identities and differences, as in Rawls. But they must accept the equality and reasonable differences of others. "[R]eciprocity can be limited by defining it as mutual recognition among all those who subscribe to the postulate of equality and who genuinely respect the integrity of the perspectives of others" (p. 252).

Building on Lawrence Kohlberg's model of "justice as reversibility" ("Justice as Reversibility," 1979) and Jurgen Habermas' communicative ethics governing the ideal speech situation (*Communication and the Evolution of Society*, 1979)—in which participants imagine themselves in others' positions and strive through disinterested dialogue (unbiased by ideology, distortion, or power) to construct models of justice—Rosenfeld attempts to show that open-minded citizens will realize that each individual needs autonomy and requisite resources in order to pursue his or her life plan with true equality of opportunity (pp. 220–31). Only affirmative action along the lines of the *Bakke* and *Richmond* dissents will provide for this opportunity. Rosenfeld's application of reversibility and communicative ethics is new in the analysis of affirmative action.

Fiscus' book is a short, mainly deductive argument in favor of the constitutional validity of the use of proportionate quotas by courts and other institutions to achieve the racial balance that would have prevailed in the absence of first order and other forms of discrimination. Fiscus admits that the universal use of proportionate quotas would be counterproductive, but he wishes to clear the path for their constitutional legitimacy whenever institutions voluntarily adopt them or when courts deem them necessary (ch. 4).

His standard is "ideal world fairness," which means that each race should have equal representation in each job in the workforce, based on the percentage of its representation in the general population. The metaphor he chooses to support this standard of equal representation ups the ante on Rawls' veil of ignorance: he compares this ideal distribution of jobs to the way that differently colored molecules would be distributed across different zones were they released into the air without any extraneous influences (pp. 20–24). Accordingly, "[o]nly racism, if not of a direct and tangible sort, can explain the failure of racial minorities to attain their deserved proportion of the society's important benefits that they would have on the basis of their numbers in the society . . . it follows that minorities have the right to claim equal proportional benefits for themselves" (p. 13). Fiscus' book clearly and forcefully carries out the logic of equal proportional representation that lurks behind the obfuscations of many writers today who are reluctant to be fully honest about their intentions.

Epstein, too, opposes reliance on the antidiscrimination principle, but he views such reliance as preferable to the judicial move toward equal impact in Title VII adjudication, for it is less interventionist than the latter. The costs of the massive regulation needed to enforce Title VII, especially with the adoption of equal impact tests, outweigh the benefits that have prevailed in terms of the consent of the governed, the effect on liberty, and economic growth. Epstein favors massive deregulation except for the constitutional rules prohibiting government discrimination and federal law prohibiting discrimination in public accomodations and transportation (due to the problem of monopoly and lack of public alternatives). Otherwise, courts should enforce only the terms of voluntary "at will" contracts made between employers and employees. Individual choice by firms would prevail, modeled on freedom of association.

Interestingly, since freedom of association would prevail in all cases outside of government and public accomodations, companies could engage in all the discrimination or affirmative action that they wanted. Affirmative action could not be mandated, nor could it be prohibited. As in the area of freedom of expression, the law would tolerate a diversity of opinions and practices concerning race relations and employment. In a revealing passage, Epstein remarks, "In First Amendment law the dominant view is that as many different forms of expression as possible should be allowed into the marketplace so that no single agency becomes endowed with the power to determine truth or mold preferences for us. Orthodoxy is a danger, not a goal" (p. 305).

Epstein buttresses his case for a market of free association in employment by reminding us of the ways in which government action and the imposition of a racist legal orthodoxy lay at the heart of Jim Crow in the South before 1954. A truly free market in employment has never prevailed.

What can we make of these different thinkers? In the Millian sense, each has the courage and conviction to carry his principles to their logical conclusion, thereby presenting us with a fully developed argument in favor of a radical resolution of the affirmative action conundrum. On the Left, Fiscus' analysis is less convincing than Rosenfeld's. It ignores historical and cultural reasons (in addition to racism) for disparate racial representation in various trades, and it never really wrestles with the political and consent problems associated with the legitimation of a quota society. Analogizing human action to the movements of molecules betrays a rejection of politics and individual integrity in favor of social engineering and a quick fix that would be nothing of the sort.

Rosenfeld understands the difficulties more profoundly, so he labors much more painstakingly to arrive at a justification for widespread affirmative action. His reliance on the ethics of dialogue is commendable, for honest dialogue can lead to deeper appreciation of counterpositions and the meaning of autonomy. But at least two problems persist. First, as with all theories that reach conclusions based on dialogues in some special state free of the pressures of politics, Rosenfeld's results appear to be manipulated to achieve the goal the author hoped to reach. More importantly, even assuming for the sake of argument that Rosenfeld's conclusions are philosophically justified, this does not mean that they are consistent with the norms of constitutional law and the consent of the governed in politics. In the end, Rosenfeld fails to show how Kohlberg and Habermas can be fitted into the constitutional tradition.

On the Right, Epstein provides a fine critical analysis of the untoward consequences of present law and policy, and his criticism of government's role in perpetuating inequality and racism in the past is enlightening. It is refreshing to hear a law professor these days argue for the extension of First Amendment norms to other areas of social life. But Epstein's reliance on an almost purely market solution is as utopic as the conclusions proffered by Rosenfeld and Fiscus. He assumes that an unfettered market would provide ample opportunities for those denied employment on account of race, but nowhere does he offer sufficient evidence to support this claim. Epstein does a better job than the other two writers of linking his position to constitutional norms, but he pays insufficient attention to the powerful consensus surrounding the antidiscrimination principle, and to the fact that the constitutional regime is not predicated simply on the liberty of markets.

In the end, each of these writers attempts to resolve the predicament of equality by offering a radical resolution which transcends the tensions between equality of condition and liberal constitutional norms. Back in the real world of politics and constitutional law, it is not clear how much help each book is, for we either have to abide by the clarity and consensus provided by allegiance to the antidiscrimination principle, or continue to struggle with Sisyphus' dilemma.

University of Wisconsin, Madison DONALD A. DOWNS

Choosing Justice: An Experimental Approach to Ethical Theory. By Norman Frohlich and Joe A. Oppenheimer. Berkeley: University of California Press, 1992. 272p. $45.00.

Everyday Justice: Responsibility and the Individual in Japan and the United States. By V. Lee Hamilton and Joseph Sanders. New Haven: Yale University Press, 1992. 290p. $35.00.

Conventional wisdom argues that the normative issues of justice are resolved by philosophical or religious discussion, not by factual descriptions of what people are like or what they actually believe. But, as these two well-presented and thought-provoking books show, this belittles the contribution of social psychology to the contemporary normative debate. Philosophical accounts of justice make very specific assumptions about human needs, beliefs, and motivations, which social psychology can test.

The centrality of such assumptions about motivations and beliefs in Western liberal theories of justice is well illustrated in the work of Rawls's that is the basis for *Choosing Justice*. Rawls tries to identify principles of justice impartially by the thought experiment of the original position, whereby rationally self-interested individuals debate the issues under a "veil of ignorance" hiding from them knowledge of their own self-interest. As Rawls has acknowledged, the device represents critical reflection on our own existing beliefs, freed from the distortion of our own self-interest. The claim that certain principles reflect our own existing beliefs, which would be chosen by consensus under the conditions of the "original position," appeals to facts about human nature that social psychology can test and supply. *Choosing Justice* sets out a series of experiments conducted to test the validity of Rawls's assumptions and those of his rival Harsanyi. The basic experiment faced test-subjects ignorant of their subsequent positions with a choice of principles of justice that were to govern the payoffs they were to receive in later earning activities. Not surprisingly, given the absence of adequate information on how they would be affected, the participants did generally come to a consensus on the principle to be applied. More surprisingly, they confirmed that choice even when they did have such knowledge and despite losing some of their earnings, in order to benefit those less well-off. In terms of the principle chosen, the result differs from Rawls's difference principle (here called the "maximum floor") and Harsanyi's maximum-income approach. Participants did wish to have a floor below which no one could fall, but that was not so great as to limit the incentive of the more productive. Although, as Rawls predicted, it was difficult to achieve consensus on such a floor, the experiments cast doubt on his own conclusions.

The authors have conducted their experiments with circumspection and care. They are careful to identify and test for potential distortions caused by the character of those chosen as participants or by the design of the experiment, but they are able to vindicate their results.

The authors wish to go beyond simply testing a specific theory through the tried techniques of justice experiments. A major lesson offered is that in the experiments, people continued to support the principles of justice when put into operation if they have participated in choosing them even when they are forced thereby to give up some of what they have earned. Productivity also was not sacrificed by such redistribution. From this, the authors conclude that participation is more likely to bring acceptance.

The experiment is valuable as a self-critical example of what people are likely to choose in conditions approximating to impartiality and as an important contribution to the debate on the assumptions which underlie some of the work of contemporary philosophers.

The most substantial claim of the authors is not simply a criticism of Rawls, but that the experimental method, designed as an exercise in impartial reasoning, is a technique for discovering the *content* of justice (p. 3). Like much social contract literature, they presume that ethical validity attaches to the results of bargaining between individuals in conditions of impartiality (see p. 92). But of course, that presumption begs a lot of questions. To be sure, we have to make certain assumptions about human interaction in society in any ethical theory; but the argument that we can build a theory of what we should do on the basis of what we, impartially, actually want to do requires more justification. Rawls's thought experiment is to take us out of the contingencies of our own beliefs, but he corrects it with the "reflective equilibrium" to make the results approximate to the considered judgments of our society. This experimental study is valuable as a basis for criticizing the outcomes Rawls posited; but it presumes that his methodology of relying on actual views, suitably screened, can properly produce a theory of justice.

One of the valuable elements in the *Choosing Justice* study is the use of different cultures to test the results. Groups in North America and Poland (then under communism) were chosen. Some differences in approach are noted between the groups, notably a greater willingness to accept redistribution and a significantly higher floor set by the Poles ($14,000, as opposed to $10,000). No attempt was made to create mixed groups where bargaining had to take place between different cultural groups. The difficulty of such an exercise is demonstrated in Hamilton and Sanders's innovative and challenging work, *Everyday Justice*. This work compares views of sample Americans and Japanese citizens on negative justice issues—allocation of responsibility and decision on appropriate punishment. The study takes, as its starting point, attitudes toward litigation and other forms of dispute settlement in the two countries. This study carefully tests the hypothesis that such cultural differences have structural underpinning in differences in the distribution of social ties (p. 88).

Differences in attitudes to justice issues are sampled by telling the same four core stories with a number of variants to sampled individuals contacted by post or telephone in two industrial cities—Detroit and Yokohama—and one more traditional Japanese center, Kanazawa. Reactions to stories set in everyday stranger relationships (street crime or a traffic accident) differ markedly from work and family incidents. In both countries, more severe judgments are made according to the deliberation with which the act is done and the seriousness of the consequences of the act. But in the United States, people were more likely to stress the importance of the individual's mental state, whereas the Japanese attached responsibility more to the status of the actors in the social hierarchy. Thus, the Japanese approach to the allocation of responsibility is more contextual than that of Americans. On the whole, the Japanese proved more willing to punish but were keener on restoring ties in family and work relationships. The study is important in counterbalancing the broadbrush approach of some studies on litigation rates as an indicator of approaches to justice by emphasizing the importance in the social culture of roles, and the very different kinds of judgments that are made about the acts of those within relationships and those outside.

The approach is important not just for what it says in significantly refining previous work on differences in American and Japanese attitudes to justice. It also calls into question the methodology of Rawls and social contract theorists who build models of justice around the rationally self-interested individual and what he or she is prepared to bargain for in conditions of impartiality. If we have cultures in which expectations about the performance of individuals in roles are crucial, then the kind of theory of justice that would be produced in a reflective equilibrium would have to give an account of justice in roles, rather than broad principles of universal justice. Indeed, bargains between effective strangers would not begin to capture the kind of ideas of justice that society would consider appropriate. By locating justice in what actual people actually believe (with suitable refinements), we seriously compromise the possibility of a universal theory of justice such as Rawls and the authors of *Choosing Justice* present.

University of Leeds JOHN BELL

Justice by Lottery. By Barbara Goodwin. Chicago: University of Chicago Press, 1992. 214p. $57.00 cloth, $16.95 paper.

Imagine a world in which jobs, incomes, and housing are assigned by lottery every five years. Extremes of wealth and poverty have been eliminated, but some live more grandly than others for a time (since variety pleases) before homes and salaries are reshuffled yet again. Political power is also allocated by lot, thereby eliminating the incentive politicians now have to tailor votes to curry support at the next election or to solicit lucrative employment upon leaving office. Is this Utopia, or does Orwell spring to mind?

Barbara Goodwin presses this question with admirable pluck in her stimulating book. She begins by describing a hypothetical European nation in the twenty-first century—Aleatoria—that doles out power, wealth, and occupations randomly while keeping even its poorest citizens comfortable and providing equal and excellent education to all (barring a cadre of experts, who receive special instruction). The remainder of the book examines the advantages and drawbacks of lotteries in various contexts, from choosing workers to selecting kidney recipients. Although Goodwin's own views are not entirely clear, she appears largely to endorse a *total social lottery*, albeit in a less rigid form than Aleatorians would favor. But she helpfully explores less radical changes that might garner broader support.

Goodwin starts from the premise that people have equal moral worth and therefore possess a right to "equality of life-chances" as part of the "equal respect" they are owed (p. 110). Goodwin does not say precisely what moral desideratum she uses to judge lotteries in contrast with other distributive mechanisms. Her failure to discuss competing ideals, such as Dworkin's notion of equality of resources or Cohen's equal access to advantage, is frustrating, because only after a goal is chosen can the instrumental merit of a lottery be assessed. Presumably, she has in mind rough equality of material resources and opportunities for meaningful work but nothing more sweeping, because she does not advocate, or even consider, requiring people who are naturally more attractive, intelligent, creative, or physically fit than others, whose lives are consequently more satisfying, to compensate those whom fortune spurns. Why these differences are morally irrelevant is far from obvious, given Goodwin's lightly defended claim that "the concept of desert itself is incoherent and, philosophically speaking, unfounded" (p. 64). These disparities are equally undeserved in a moral, rather than an institutional, sense and equally important determinants of "human well-being, broadly interpreted," which Goodwin regards as "the final validating principle of any theory of social justice" (p. 102).

Notwithstanding this complaint, Goodwin conjoins to her claim about equal entitlement four further premises: (1) that moral desert is illusory, or at least that rewarding desert is too difficult because desert is hard to measure; (2) that people's abilities are approximately equal; (3) that the abundance of discrete, noninterchangeable goods and opportunities precludes making everybody's situation the same; and (4) that people are fairly risk-averse, leading them to favor a narrow band of incomes and a relatively high minimum. She concludes that people are treated as equals if they are given an equal chance, in separate and independent lotteries, at assorted jobs and a spectrum of wages and places to live. She further contends that people would prefer diverse salaries and living conditions to equal incomes and standardized housing.

These assumptions are controversial, as are the conclusions Goodwin draws from them. Many will find Goodwin's replies insufficiently detailed and nuanced to persuade. For example, if people savor variety as much as Goodwin believes, why do the talented and socially privileged among us not change jobs as frequently and dramatically as participants in the total social lottery? The satisfaction in honing talents, displaying expertise, and pursuing stable careers might be greater than Goodwin admits. And even if desert is a fiction, why should not someone be free to earn more by working overtime than somebody who prefers more leisure to additional possessions, especially if the first shares some of his or her product with the second, leaving both better off? It is also puzzling why everyone should be forced to participate in an income lottery, when a Pareto-superior alternative is available that seems a truer expression of equal treatment: give everybody the same salary and allow those who want a wider income spread to gamble among themselves.

Goodwin neglects many problems of implementation, as well. She argues that people should be allowed to pursue occupations "for which they have some calling" (p. 84). But complete freedom is plainly impossible, because if everyone could do as they pleased without jeopardizing their incomes, we would have too many film stars and not enough farm hands, many jobs would not get done, and the supply of goods and services would bear no relation to what consumers want. Central planning, it is conceded, would be necessary; yet its track record is not encouraging. How people could become experts with lifetime jobs and which positions would be so designated is also unclear. Nor does Goodwin discuss at any length the need for incentives to maintain housing, innovate, work diligently, preserve productive capital, and save for the future, especially when bequests are sharply limited (though she notes possible difficulties arising therefrom). History offers lessons about these matters (the former Soviet bloc is instructive), and most of them are gloomy. Likewise, while there is much to be said for eliminating career politicians, it is a separate question whether randomly chosen leaders would better advance social welfare than, for example, elected representatives with limited terms who were denied subsequent rewards by interest groups.

These misgivings could be multiplied. The great merit of this book is that it insistently and acutely challenges readers to say what our moral equality justly demands even if one balks at Goodwin's own answers.

University of California, Berkeley ERIC RAKOWSKI

Rethinking Obligation: A Feminist Method for Political Theory. By Nancy J. Hirschmann. Ithaca: Cornell University Press, 1992. 365p. $39.95 cloth, $15.95 paper.

Two key assumptions ground almost all liberal theories of political obligation. One is that government authority threatens the primacy of individual freedom. The other is that if we want to justify government authority, we will have to show that such authority has its source in our own free choice. In *Rethinking Obligation,* Nancy Hirschmann questions both assumptions and goes on to develop a more coherent way of thinking about our political obligations, one that requires her to extend the insights of feminist standpoint theory. The result is a highly intelligent and clearly written book, one that should be of great interest to all political theorists, as well as to all those concerned about the gendered nature of our political concepts.

Hirschmann's primary goal in the first part of the book is to show that consent theorists of political obligation are of two minds with regard to government authority. On the one hand, they explicitly deny the primacy of government authority when touting the value of free choice. On the other hand, they implicitly accept the primacy of government authority when asking why individuals should freely consent to it. Not surprisingly, their implicit acceptance of such authority is deeply problematic, since it both renders consent theory contradictory and denies the possibility of that which consent theorists value most highly: free choice.

How do consent theorists get themselves into such a bind? According to Hirschmann, while they are correct to notice a tension between individual freedom and government authority, they mistakenly construe such a tension as a matter of contradiction. Hence, they are forced not only to present government authority and individual freedom as polar opposites, but to view government authority as a violation of individual freedom. Hirschmann argues that such oppositional thinking is at least partly the result of masculinism, which she defines as an ideology that reifies, exaggerates, and privileges certain aspects of men's historical experiences at the expense of taking women's historical experiences seriously.

What do consent theories have to do with male experiences? How could they possibly exclude the experiences of women? Hirschmann claims that consent theories are masculinist in at least two important respects. First of all, they assume that consent creates obligation, an assumption that precludes the possibility of calling a nonconsensual relationship an obligation and that thereby obscures the nonconsensual obligations that pervade women's lives. Second, they embrace a separation between the private and public that is not only grounded in women's powerlessness but conducive to the development of oppositional thinking.

Hirschmann explains both aspects of masculinism by zeroing in on the quest by young boys for separation from their mothers, a quest that leads them not only to embrace the values of self-control and independence but to assert the primacy of natural rights and universal laws when theorizing about politics: "Under natural rights, one reasons not from one's innate sense of connection to others but from 'laws' that 'naturally' exist independent of all men" (p. 214). Hirschmann argues that such independence from social relations is not possible. Nor is it something that we should aspire to, since it "causes relationality and connectedness to become subverted in both theory and practice" (p. 141).

While the female model of relatedness is far from perfect, it does, according to Hirschmann, provide us with a better framework for analyzing our political concepts, since it emphasizes human connectedness at the outset and construes individuality as part of social relations. What does this mean for the concept of political obligation? Hirschmann argues that if we want to redefine political obligation to accommodate women's experiences, as well as men's, we will have to cease pretending that we create our obligations and begin construing them as "givens." Likewise, we will have to stop looking for the origins of our obligations and start asking whether or not we want to alter them in some fashion.

Hirschmann does not, as it turns out, spend a great deal of time on the content of our present obligations or on what it would mean to change them. Instead, she concentrates on showing both why her conception of political obligation entails participatory democracy and what a feminist method for doing political theory might look like more generally. While both discussions are very stimulating, three key questions remain unanswered about her model of political obligation itself: (1) How closely tied is her model to women's experiences? (2) How, if at all, would the practice of obligation change once participants began to think of obligation in the way that she does? and (3) What would it mean to be obligated to an abstract state—which ostensibly transcends particular relationships?

Since Hirschmann makes clear that men's obligations are no less "givens" than women's, she can use her model to explain men's obligations even though, in doing so, she has to separate her understanding of those obligations from men's own understanding. But, we have to ask, what sorts of changes would have to take place in the world before men could understand their obligations contextually without denying their own experiences? What, moreover, would it mean to be obligated once obligation ceased to be associated with consent? Would it still connote an act of promise? Honor? If not, what difference would there be between obligation and responsibility?

What, furthermore, would obligation mean with reference to institutions such as law? Hirschmann provides us with a firm understanding of obligation in participatory democracy, where individuals are related to each other directly. But what about institutions that are both mediational and necessary to participatory democracy, such as law? Can we be obligated to obey such institutions? If so, how? If our obligations to such institutions flow from a prior agreement among community members, how can we legitimate them without reference to their consensual origins? All of these questions are important to understanding the practice of political obligation. Hirschmann does not provide us with complete answers to them. But she does provide us with a framework for arguing about them that is both original and very well thought out.

University of Wisconsin, Madison MARION SMILEY

Ideals as Interests in Hobbes's "Leviathan": The Power of Mind over Matter. By S.A. Lloyd. New York: Cambridge University Press, 1992. 396p. $54.95.

The Two Gods of "Leviathan": Thomas Hobbes on Religion and Politics. By A.P. Martinich. New York: Cambridge University Press, 1992. 430p. $59.95.

A significant revival in the study of Hobbes's political theory has been underway from the mid-1980s onward. One facet of this revival has been a new effort to take the text of Hobbes's masterpiece, *Leviathan,* seriously as a whole. Both these books continue that effort but with quite different degrees of success.

Sharon Lloyd's argument begins from recognition of the fact that human beings are interested in things other than their own preservation and comfort. Some of these interests, she points out, are "transcendent," that is, they (sometimes) take *priority* over our interest in self-preservation. Lloyd argues that Hobbes recognized the existence of transcendent interests and, in fact, accorded those interests a central place in his political theory. This claim is the basis of an interpretation of Hobbes's political theory that is sharply at odds with views held by most of Hobbes's interpreters. Most interpreters believe that Hobbes relied on fear (in particular, fear of violent death) to motivate subjects to accept rule by an absolute sovereign and on the overwhelming coercive power of sovereigns to maintain their subjects' obedience. Lloyd points out, however, that Hobbes's account of the causes of disorder emphasizes "passionately held opinions concerning [subjects'] transcendent interests" (p. 44). Since these opinions are at the heart of his account of the causes of disorder and civil war, they are also at the heart of his proposed solution to the problem of disorder and civil war. Hence, "[it] is education, and not might, that makes for social order in Hobbes's system" (p. 2). Most importantly, subjects must be taught that their transcendent interests in salvation and in their duties to God are compatible with acceptance of any effective sovereign power that may happen to exist in the territory in which they reside. According to Lloyd, Hobbes undertakes the necessary educational task by "redescribing" those transcendent interests held by many Christians in his time in an effort to demonstrate their compatibility with subjection to a sovereign.

Lloyd's point of departure—namely her claim that Hobbes recognized and considered important the fact that people do not always give their interest in self-preservation priority over all their other interests—is on target. In fact, as she demonstrates, it is impossible to make sense of numerous passages in Hobbes's writings if one rejects this claim. But the interpretation that she develops of Hobbes's political theory as a whole, on the basis of this claim, contains many flaws. Both her rendition of Hobbes's account of the causes of civil war and her version of his solution to the problem are misleading.

In Lloyd's view, Hobbes believed that "passionately held opinions" are the causes of civil war. In the English Civil War, these opinions were religious in nature. But this ideological interpretation of Hobbes's view rests on an excessively selective reading of his work. In Hobbes's general account of the factors that tend to lead to civil war, seditious doctrines or opinions play an extremely important role (*Leviathan,* ch. 29). The *first* factor Hobbes mentions in that account, however, is insufficient sovereign power. Hobbes suggests there that William the Conqueror committed a critical error when, at his reception in England, he took an oath not to infringe the liberty of the (Catholic) Church, thus allowing a portion of the sovereign power that should rightfully have been his to be held by others. Lloyd is right to notice that according to Hobbes, passionately held opinions are an important factor in fomenting disorder; but she writes as if it were the sole factor (or at least the factor of overwhelming importance), neglecting Hobbes's claim that it is crucial for a sovereign to retain all of his rights.

Lloyd's account of Hobbes's solution to the problem of disorder is similarly one-sided. She correctly points out that according to Hobbes, overwhelming coercive power is not *sufficient* to maintain subjects' obedience. Yet frequently, she seems to go further. She states, for example, that in Hobbes's theory, "Opinion, *not* might, makes order" (p. 39, emphasis added). In the end, Lloyd concedes that coercive force plays a role in Hobbes's prescription for the problem of disorder; but even then, she insists that the "*primary* role of force is to aid in the reformation of the universities" (p. 211, emphasis added). Hobbes did believe that reformation of universities was badly needed, but it is misleading to suggest that the primary role of coercive force in his theory was to accomplish that reformation. Similarly, Lloyd consistently downplays the role played by fear in Hobbes's political theory despite the fact that she is aware of Hobbes's claim that the principal reason for which people live in commonwealths is to secure protection for themselves and cites his plain statement in *Leviathan* (Macpherson ed., 1968, p. 200) that "The Passion to be reckoned upon, is Fear." Her argument against the view that Hobbes believed that people are *always* more interested in their self-preservation than in other things is sound. But she greatly exaggerates the role played by motives other than the desire for self-preservation in Hobbes's solution to the problem of disorder.

One of Lloyd's central concerns has to do with Hobbes's politics of scriptural interpretation. According to her, one of Hobbes's objectives in *Leviathan* was "to correct through a process of redescription and reeducation the beliefs that when passionately held underlie socially disruptive activities" (p. 45). Her focus is on *religious* beliefs, including superstitious fear of spirits and other opinions that Hobbes considered inimical to the power of civil sovereigns. Lloyd acknowledges that this redescription project "may involve substantive revision of views" (p. 345, n. 9); but she insists repeatedly (in opposition to the "cultural transformation" thesis) that it is not credible to suppose that Hobbes might have intended to persuade anyone to *abandon* their beliefs. According to her, Hobbes sought merely to "rationalize" Christianity.

I agree that Hobbes sought to rationalize Christianity. But Lloyd fails to consider the fact that in view of the sorts of beliefs that many Christians in the mid seventeenth century held to be central to their religion, the rationalization of Christianity must necessarily have appeared to entail its abandonment. Catholics, Presbyterians, and many independents interpreted Hobbes's reading of the Bible as what it was—an all-out attack on their religious views, not a plausible effort to redescribe them.

If Hobbes wanted Christians to transform their beliefs (not necessarily in a single generation and certainly not in a shorter time), does it follow that he *intended* to lead them to abandon Christianity altogether? Not necessarily. In his rich, well-conceived, and historically informed study focusing on Hobbes's religious thought, Aloysius Martinich explains why.

Martinich argues that the aim of Hobbes's writings on religion was not to subvert Christianity but to provide "a new theory for Christianity, a theory that would make it compatible with the modern science of Copernicus, Galileo, and Harvey" (p. 7). Similarly, his writings on morality were intended neither to overthrow morality nor to replace an old morality with a new one but, instead to provide a new theory to explain the basis of morality. In Martinich's view, the *content* of Hobbes's interpretation of Christianity was strictly orthodox, just as the content of his moral theory was strictly conventional. What was new was the *theory* that Hobbes offered to account for that content. Martinich asserts that "Hobbes was bent on initiating a cultural transformation" (p. 5). Ultimately, Hobbes's efforts led toward secularization and atheism, consequences that, he argues, were not intended by Hobbes, though the rationalization of Christianity and its reconciliation with modern science were.

Martinich's argument seems to me less than fully persuasive at several significant points. For one, even if his main argument is correct, I doubt that God really plays as central a role in Hobbes's moral and political theory as Martinich believes (pp. 44, 135). More generally, I am not convinced that Hobbes should always "be taken at his word" as readily as Martinich suggests (p. 43). Martinich seems insufficiently to appreciate the fact that Hobbes was a *political* writer (not just a writer *about* politics) and that *Leviathan* was a work of *political* speech. Political writing aims to change people's minds, nor does it always rely on plain speech and purely rational persuasion to accomplish this end. As an academic philosopher, Martinich seems less sensitive than I think readers of Hobbes ought to be to the possibility that Hobbes may have been using argumentative strategies that are not fully transparent.

These reservations are mere quibbles, however, in relation to the value of this book. *The Two Gods of "Leviathan"* is the most systematic examination of the theological elements in *Leviathan* in the modern literature on Hobbes. Martinich has situated Hobbes's theological and ecclesiological views in relation to their sixteenth- and seventeenth-century context more thoroughly than any other recent interpreter. He offers plausible reasoning that reconciles the boldness and innovative character of Hobbes's theological *arguments* in *Leviathan* with the relatively conservative, Calvinistic *doctrines* expressed in that work. His work should be required reading for anyone seriously interested in Hobbes's views on religion.

Columbia University DAVID JOHNSTON

Moral Differences: Truth, Justice and Conscience in a World of Conflict. By Richard W. Miller. Princeton: Princeton University Press, 1992. 396p. $65.00 cloth, $16.95 paper.

Moral philosophy, though mutually and deeply intertwined with political philosophy, can be, and often is, practiced in such a specialized manner and made dependent on such an arcane vocabulary that it becomes virtually impenetrable to the nonspecialist. At first glance—and, indeed, after many glances—Richard W. Miller's *Moral Differences* seems to be such a book. Yet surprisingly, I find myself wanting to recommend it to political philosophers and even to urge it upon them. Not only are its concerns, finally, our concerns, but it makes an original contribution to political philosophy on the latter's master concept of justice, a contribution that should become the subject of discussion and commentary.

To be sure, this book probably should not be read as one ordinarily reads a work of political philosophy. At the conclusion of a brief introduction, Miller tells his readers: "Although the later discussions of justice and of conscience take place within the framework of the earlier account of truth and justification, most of the later arguments do not assume earlier conclusions. So if need be, one can enter this book at midstream" (p. 9). For most nonspecialists, that warning will be precisely on the mark; and I would advise them to enter Miller's project at the beginning of chapter 6 and proceed from there, returning to the beginning to encounter the pleasures—but frequently also the pains—of rigorous and often technical philosophical discourse without necessarily attempting to work out how the discussion of social justice depends (if, indeed, it does) on what has gone before. (This is also the judgment of a distinguished practitioner from the discipline of philosophy itself.)

In fact, as it happens, Miller's initial chapter, too, should be of interest to political theorists of every kind. It proceeds from a discussion of Aristotle on heroic virtue and of the unique evidentiary rules of the Tiv (an African people studied by Paul and Laura Bohannon) to a so-called mixed verdict on moral realism (or, conversely, on moral relativism). Miller's conclusion is that for example, both our own other-directed and Aristotle's inner-directed accounts of moral virtue are "true" in the sense that each is part of a consistent worldview (or moral language) and that neither would be undermined by knowledge of the "facts" on which the other bases its contrary assertions. This discussion is persuasive and might be very useful in disposing of certain kinds of hoary epistemological dilemmas in political theory discourse; but this mixed verdict does not, as Miller says, seem to be a necessary grounding of the discussion of social justice that is his major political concern.

In contrast to what becomes a very convoluted discussion of the mixed verdict, Miller's presentation of the theory of justice is engagingly simple (but very far from simplistic). How to derive a single conception of justice for a "world of conflict"? Miller begins, unexpectedly, with a kind of rueful but unequivocal capitalist triumphalism: "In an advanced industrial setting, some reasonably efficient capitalist system is best for everyone who is constrained by commitment to justice as social freedom" (p. 188). (The theory of justice is, compatibly with the mixed verdict, not universally recommended;

but it is universally recommended for "advanced industrial" settings.) On the other hand, in this setting "people are, pervasively, victimized by social barriers to advancement in any reasonably efficient capitalist economy" (ibid.). The derivation of a theory of justice is, then, *internal* to capitalism itself; we do not subject capitalism to standards external to it. However, just because "justice as social freedom" is the standard by which capitalism is morally triumphant, the "barriers to social advancement" that capitalism necessarily erects and depends on must be reformed in the name of the very same standard that justified capitalism in the first place. For example, the capitalist labor market demands that in the name of efficiency, some citizens will be treated as the means to others' ends; since such treatment is incompatible with the requirements of "justice as social freedom," compensatory or rectifying programs of social intervention are equally commanded.

What is most striking about Miller's approach here is that unlike most normative political philosophers, he proceeds to his general theory of justice from an intensely *empirical* analysis of capitalist inequalities, especially of how they are necessarily generated by the operations of capitalism. In sum, the principle that justifies "capitalism" then also justifies a kind of social democracy that looks like Sweden at a minimum and on occasion goes some way toward meeting the moral requirements not only of a serious feminism but also of a Marxism that has earlier, apparently, been jettisoned. Capitalism is triumphant; and in the name of that triumph, everything that say Friedman or Hayek or Nozick consider to be "capitalism" is rejected.

Clearly, this entire enterprise depends on the notion of justice as social freedom, which Miller defines as follows: "The institutions of a society are just if and only if they could not be rejected, freely and rationally, by anyone living in the society who desires such acceptability on the part of all who share this desire" (p. 185). Although Miller works hard to distinguish his approach from that of John Rawls, it seems, like Rawls's, to be a rational choice theory; the most significant distinction is that the hypothesized motivation of rational social actors is social cooperation, rather than self-protection. In this respect, Miller's basic assumption about what constitutes "society" and social action is even farther from that of an invisible-hand theorist like Nozick than from that of Rawls. Inasmuch as Miller's chief intellectual antagonists will be libertarians such as Nozick, he is aware of this problem for the would-be universalism of his theory of justice; and he attempts to disarm invisible-hand and social contract theorists of natural rights with a proviso that he calls "the Hobbesian interpretation": "A just system [is] a system that gives everyone a noncoercive rationale for setting aside her right to resist or rebel provided that she would agree to any arrangement yielding a similarly noncoercive rationale to all" (p. 214). It remains to be seen whether contemporary natural rights theorists will consider themselves disarmed or will rather simply reject the definition of "justice as social freedom" and "the Hobbesian interpretation." Anyone who, despite the experience of a real human world that endlessly mocks the notion of a prepolitical individual with prepolitical rights, can still confidently interpret "justice" merely as noninterference with such rights by political institutions and ignore the devastation of real individuals by the structure of "nonpolitical" market action will probably be untouched by Miller's argument. Many others, however, will find his work, especially in its detailed empirical consideration of the facts of capitalist inequality, not so much a correction of, as a necessary addition to (and perhaps even a significant alternative to) Rawls.

Smith College PHILIP GREEN

Freedom and the Organizational Republic. By Larry M. Preston. New York: de Gruyter, 1992. 235p. $102.10.

Preston's is the sixth volume in de Gruyter's Studies on North America Series and the first to approach matters from the perspective of the political philosopher. Specifically, Preston is anxious to examine our conceptions of *freedom*, and, in so doing, suggest how we need to alter both our concepts and our practices so as to secure the prospects for liberty. Let it be said that Preston approaches this task with a certain cautious optimism: "So long as the capacity to deliberate has not been entirely dulled by the promise of untold affluence, the illusion of individual autonomy, or nostalgia for a distant past, the possibility of constructing a politics of freedom remains" (p. 209). This sentence is revealing. It isolates the sources of Preston's current dismay, expresses his residual hope for the future, and reveals, after a fashion, the core elements of Preston's agenda. For Preston, it is the "illusion of autonomy" that resides at the heart of liberalism. Allegedly, this illusion undergirds the recent works of Rawls, Rorty, Flathman, and Galston every bit as much as it does the earlier efforts of Locke and Adam Smith; and it does so despite the best efforts of modern liberals to escape the charge that liberal theory is inherently "unsituated." The liberals' quest for autonomous choice and their deference to things like "rights" purportedly blinds them to the necessary conditions of freedom.

If it is the pursuit of autonomy that disables the liberal, then it is "nostalgia for a distant past" that undoes the communitarian. The latter, Preston avers, is caught up not simply by the desire to assert the situatedness of persons but also by the wish to see these persons as "free" precisely to the degree that they act in accord with the norms constitutive of their respective communities. Such an understanding of freedom, Preston suggests, goes too far.

If a meaningful measure of freedom is to be found in neither liberalism's autonomous individual nor communitarianism's situated agent, where does it reside? For Preston, the answer is that freedom is possessed by those who have the resources and skills necessary for deliberative choice. In short, freedom is not experienced in the disembodied way that liberalism suggests. Rather, it is had by individuals living within concrete historically and culturally defined circumstances. More to the point, it is had by individuals with the requisite opportunities and resources for deliberation and action, given the language and practices of their particular world.

To this point, Preston's argument is both familiar and unexceptional: his attacks on liberalism and communitarianism are well known. So, too, is his suggestion that the exercise of freedom has its preconditions. Uncommon, however, is how Preston proposes to foster freedom. Since being free requires resources and freedom ought to be secured for all on equitable grounds, liberal, marketplace capitalism must be abandoned. The claims of capitalism's apologists notwithstanding, the market is not a bastion of freedom; the goods it distributes are only given to the few who possess resources sufficient to lodge effective demands. All too many lack these resources; all too many are, therefore, unfree. Hence, freedom is only practicable within a socialist regime.

But not any socialism will do. Freedom also demands the possibility of deliberative choice made within a comprehensible environment—an environment, too, that is reasonably susceptible to individual influence. Fortunately, most of us conduct our public lives within large organizations, bureaucracies of one sort or another. If freedom is to be secured, it must be gained within a world composed of such organizations. Of course, bureaucracies have usually been seen as a threat to freedom; but this view is mistaken. As Preston sees things, a refashioned, though still bureaucratized, world holds out the promise of free choice—greater free choice, in fact, than has heretofore been available. Within our own bureaus, each can acquire the specialized knowledge and skills required for deliberation; and each can exercise effective influence, even if the scope of such influence seems limited. In short, Preston sees the best prospects for freedom in a world of organized, bureaucratic socialism—in his terms, a "republican socialism." With marketplace capitalism and its denial of equal resources replaced by a socialism dedicated to the public welfare and with organizational life so constituted as to foster deliberation, freedom can flourish. In such an environment, it may also be possible to avoid the relentless quest for "untold affluence" that has led us to submit both to the false promises of the marketplace and to the overly stratified structure of contemporary bureaucracies.

Does Preston's argument succeed? This question has no easy answer. For those who share Preston's dedication to Rousseau, as well as his occasionally "romantic" bent, Preston's "republican socialism" will have an appeal. Even communitarians may think it acceptable. After all, "republican socialism" incorporates a strong communitarian bent. Moreover, Preston's assault on communitarianism is itself rather half-hearted. His attack is directed only at those who demand unflinching adherence to extant communal norms. Preston's remarks notwithstanding, there are many communitarians who refuse to take things this far.

Liberals, on the other hand, are likely to be rather disconcerted by "republican socialism." They will think his portrait of liberalism to be something of a caricature. They will find Preston's way in distinguishing between the private and public spheres to be far too slippery—indeed, far too dangerous—to be accepted with equanimity. Seemingly limited in scope at some moments, public power is at other times said to reach as far as the community desires or to include anything that may affect the public. In short, there is no secure private realm; nor are there any things called "rights." In the end, then, liberals will wonder whether Preston is really after freedom. Is it liberty that he seeks, or the harmony of a well-ordered community? Is it a community that suppresses the bump-and-grind and sometimes tragic experiences that attend the practice of free choice in a liberal regime? If so, liberals will likely consider Preston's "republican socialism" to be a substantial retreat from the hard-won gains of the last several centuries.

Clarkson University J. P. GEISE

In Their Best Interest? The Case Against Equal Rights for Children. By Laura M. Purdy. Ithaca: Cornell University Press, 1992. 262p. $32.50 cloth, $12.95 paper.

This is a thoughtful book and, because so little has recently been written on the subject of children's rights, an important one. Hillary Clinton's now notorious articles from the 1970s spoke to a decade of theoretical concern and international activism for children's rights. Given the Clintons' recent accession to the White House, this sustained focus on the rights and wrongs of children, which disappeared in the 1980s, will almost certainly return. For those preparing to grapple anew with the problem of what we, both as individuals and as a polity, owe the next generation, Purdy's book offers a thought-provoking introduction to the philosophical side of the debate.

Purdy takes as her target the so-called "kid-libbers" of the 1970s and 1980s—those advocates of equal rights for children who, Purdy says, "believe that a child's desire should never be overridden except when that of an adult would also be overridden" (p. 99). Her book sets out to prove this view wrong by showing, inter alia, that it incorporates an inadequate moral theory, relies on a mistaken model of human development, confuses the relevant criterion for granting equal rights (it is not a shared "capacity for instrumental reasoning"), and requires social changes that are both wildly utopian and undesirable.

Allied with this critique is Purdy's counterargument: children are not suitable candidates for equal rights, because human beings need an extended period of protected learning, not just to develop rationality but to develop moral character. True justice requires that we raise caring individuals who can subordinate their interests to those of others, an end that cannot be achieved unless adults are allowed the authority at times to "limit children's freedom against their will" (p. 219). Children must be taught self-control, critical thinking, and the "enabling virtues" of "rationality, hard work and the desire for excellence" (p. 45). Admission to full adult rights will depend on their demonstrating "the capacity for planning systematic utility-enhancing projects"—by which Purdy means, generally, "having a rational life-plan" (p. 27).

Purdy concludes her argument by proposing a set of reforms that are almost as radical as the liberationist proposals she decries. If the paternalistic treatment of children is to be legitimate—if we are to veto children's immediate wants without injustice—then we, as parents, must be sure we know "*this* child's interest" (p. 164). Just treatment of children requires sustained selfless attention to their developing selves so that when we do override their stated desires, we do so confident that we know our children better than they do themselves. In addition to imposing on both the poor and the yuppie "a considerably higher standard of parenting than is now considered adequate" (p. 164), Purdy envisions significant social changes so as to make the standard easier to meet. She mentions, specifically, equality of the sexes and a reorganization of the work world "so that everybody has access to a job that pays a living wage and allows for enough leisure to parent successfully" (p. 232).

Purdy pursues her wide range of points with candor, clarity, and theoretical sophistication. Particularly welcome is her familiarity with empirical studies of child development. But her expertise aside, how successful is the case she makes against equal rights, and how persuasive are the proposals she offers in the name of a "better protectionism"?

Equal rights, in Purdy's version, means ceasing to treat children paternalistically in any way—an end to any adult thwarting of a child's will. But the very few child advocates who argue in this uncompromising (and unpromising) vein are not a very interesting target. One can support the Equal Rights Amendment without proposing that we abolish gender as a social category. Why should pursuing equal rights for children necessarily mean the elimination of any concept of "childhood"? Purdy's extreme and unsympathetic definition leaves one suspecting that there remains a case for equal, or almost equal, rights for children that she has not yet answered.

Purdy's constructive argument also runs into difficulties. She is right to say that children, while demonstrably rational, lack the cognitive and critical thinking skills necessary to judge and act upon their interests. (The problem is not that my five-year-old doesn't know she needs to eat dinner; it's that she cannot yet negotiate cooking a meal for herself.) Children are thus necessarily dependent and properly under the authority of parents and other adults. The problem, however, is that adults are not necessarily or consistently good judges of children's best interests.

Purdy responds to this potential injustice by calling for higher parenting standards, greater attention on the part of the powerful (parents) to the interests of their subordinates. Here, she misses the central reason that child advocates are driven to call for equal rights—the insight that power corrupts not only in government but in families, as well. Because they are in positions of authority, parents cannot be counted on always to act in their dependents' best interest, however responsible they are called upon to be. Children, like adults, need rights to ensure that their interests are properly attended to; and they need rights they can exercise not only against the state but also against their parents.

Purdy needs a stronger conception of parents as trustees of children's interests, with their authority tied to a satisfactory discharge of their responsibility to meet children's basic needs. As children develop the necessary competencies to exercise different rights and freedoms, the trustees' role would be correspondingly scaled back. While this may not be a case for *equal* rights for children (especially as Purdy defines it), it is one for extensive rights. Purdy seems sympathetic enough to this position that had she not chosen to focus her attack on a cadre of essentially straw men, her argument might have been taken as the opening salvo in a new round of efforts to secure for children the rights that are theirs.

Yale University SHELLEY BURTT

The Heidegger Case: On Philosophy and Politics. Edited by Tom Rockmore and Joseph Margolis. Philadelphia: Temple University Press, 1992. 448p. $49.95 cloth, $29.95 paper.

This volume is composed of 18 essays by Continental and Anglo–American authors; most were written for the volume. All appear for the first time in English, and those that have appeared before are not readily available even to those readers who have the necessary language skills. The translations read smoothly. With very few exceptions, the essays are of excellent quality, morally and philosophically serious.

The impetus for the volume is quite clearly the publication in 1987 of Victor Farias' *Heidegger and Nazism.* The 1989 English translation of Farias' book was effected by the editors of the present volume. Farias' book had many problems (it was ad hominem and philosophically thin at best), but it did place the fact of Heidegger's participation in National Socialist institutions and beliefs squarely on the critical table. Collectively, the essays about this volume thus raise, sometimes explicitly, the question of the "relation [of the philosopher] to political and social realities" (p. 364). I should like to try and give my understanding of the importance of the problems that occasion these essays. I was particularly impressed with Rainer Marten's "Heidegger and the Greeks," Nicolas Tertulian's "The History of Being and Political Revolution," and Hans Christian Lucas' "Philosophy, Politics—and the 'New' Questions."

A number of agendas have come together over the questions raised by Farias' book. The opposition to French antihumanism, be it in its Marxist Althusserian or so-called Lacoue-Labarthian postmodernist versions, claims to find vindication in an association of Heidegger's own nonhumanism with his Nazism. (A sympathetic account of this can be found in Rockmore's chapter "Heidegger's French Connection.") Additionally, thinkers like Richard Rorty, whose position is most closely approximated here in the essay by Dallmayr, have argued that our rejection of the possibility of an adequate universal epistemology does not require rejection of the institutional structures associated with that epistemology. They conclude, that one can bracket Heidegger's politics without being obliged to dismiss his philosophy.

One cannot do this easily. Almost all of these essays, however, start from the position that one cannot simply separate Heidegger the thinker from Heidegger the Nazi party member and rector of the University of Freiburg. Consequently, they do not think it sufficient to say (as did Farias) that Heidegger should have simply denounced his attachment with Nazism, a position that, incidentally, makes the same presuppositions as do those who see two Heideggers (see, e.g., the essays by Dallmayr, Caputo, and Janicaud). These essays, then, insist that the reader needs to insert a properly critical wedge so as to retain "appreciation for what remains valid in his thinking" (p. 84).

Let me suggest two issues that "the Heidegger case" gives rise to that are by and large touched on only tangentially in these essays. The presumption in the passage from Zimmerman's essay is that what is "valid" is not what is Nazi. We do not think, for instance, that Newton's scientific investigations are called into question by his extensive astrological work. What is "valid" in Newton is the science, and the presumption is that the boundary between science and astrology is clear. (Recent work casts some doubts on the validity of this stance, I should note.) This points us at the first question that the Heidegger case raises or should raise. What does it mean to call thought "valid"? More broadly, what does it mean to say that a man who is arguably (with Wittgenstein) one of the two greatest philosophers of the century was a Nazi—and not just by accident (in passing, as it were) but a thinker who remained committed to the "inner truth and greatness of National Socialism" (p. 243). I must here question Taminaux's designation of Heidegger's position as a "compromise with Nazism" (p. 199). If all that is involved here is that Heidegger "compromised" or was "politically ignorant" (p. 248), well, we do know what to say about such fools. Even those sympathetic to Heidegger's thought (Dallmayr gives a useful reading of some of them) indicate that Heidegger's attachment to Nazism has to do with "failures" in his thought (e.g., failure to rid himself completely of metaphysics).

Do we know what to say about Heidegger? I cannot in this space show credibly that the ignorance–compromise–failure reading is wrong. But let me assume it is wrong, for the purposes of investigating what it means to declare that thought is valid. If we allow that, then a first issue can be phrased, "Were the Nazis (intellectually) smart?" A standard response might be: "No! Nazism appealed to the emotions, to resentment, to pride, to blood. Those who succumbed did not have sufficient resources in their reason to resist this appeal. There is not really any such thing as Nazi *thought*."

From this picture, it would follow that either Heidegger's thought is not centrally infected with his Nazism or that if it is, it is not really thought—certainly not "valid" thought. What kind of claim might one look at here? Take Heidegger's comment about modern technology: "Agriculture is now a motorized food industry—in essence, the same as the manufacturing of corpses in gas chambers and the extermination camps, the same as the blockading and starving of nations, the same as the manufacturing of atomic bombs" (p. 266). A first reaction is that this is morally and conceptually horribly wrong; but the matter is deeper. As Caputo argues in his essay, Heidegger typically uses natural objects (jugs, bridges, the earth) as examples of the thought of Being. The passage about technology operates on the assumption that the category of those who are victims of modernity can make no claim—have no availability—to Being. Valid thought, for Heidegger, is thought that makes the world as it is (i.e., in its being) available. The thought of the world is not, for Heidegger, the thought of another person. The above passage is horrific if you want the *world* to be about *human* beings, that is, if you conceive of human beings as constructing and identifying themselves by means of the world.

There is, thus, a second issue raised by the Heidegger case. Typically, the West has thought of human beings as constructing themselves in terms of their activity. This is, one might say, what is meant by "identity." Heidegger wants to resist all forms of the notion of identity and its attendant valorization of subjectivity. (Rainer Schurman's essay is first-rate on this, if a bit dense in language.)

Some of the essays in this collection raise this problem; but none, as far as I can tell, even implicitly begins to suggest what would follow from this understanding. The issue arises if one can think about politics *at all* unless one thinks about it in terms of subjects and agency. Some affirmative answers to this question have recently been given elsewhere by some feminist political thinkers (Butler, Brown) and recent books problematizing the question of identity (Connolly, Kateb, Corlett).

Does this recent work imply that one can pick and choose in Heidegger, in the manner recommended by some of these writers? I do not think so. The implication is rather more threatening: it means that Heidegger was on to something and (for whatever reason) thought he saw it becoming concrete in the political developments of the early 1930's in Germany. I do not think that Heidegger made a "mistake" in embracing Nazism: it was one possible manifestation of his thought. In this sense, Theodor Adorno was not wrong when he proclaimed that *Being and Time* was Nazi to its core. Contra Rockmore, I would hold that "Heidegger's discussion of art or technology" *does* illuminate Nazism (see p. 395). The danger and the demand is that they illuminate much else also. I do not think one can a priori make impossible their illumination of Nazism while holding on to the as-yet-mostly-unexplored "much else." Thinking is dangerous, which is one of the reasons that we have recently seen a deluge of attacks on philosophy. The political and philosophical question implicit in these essays—but never raised and never answered—is whether one should run the risk of thought, especially if one can avoid it.

University of California, San Diego TRACY B. STRONG

Collective Action: Theory and Applications. By Todd Sandler. Ann Arbor: University of Michigan Press, 1992. 237p. $42.50 cloth, $15.95 paper.

Todd Sandler has written a systematic review and critical analysis of the vast literature on the collective action problem produced since the publication of Mancur Olson's classic, *The Logic of Collective Action* (1965). The book is an accomplished blend of theory, analytical applications, and empirical evidence. For newcomers to this literature, it will serve as an admirably thorough and accessible introduction to a surprisingly complex field. For scholars who have substantial knowledge of the literature, it might not provide many new theoretical insights; but it will offer an interesting and comprehensive source for the growing work, both theoretical and empirical, on these questions.

Sandler presents the main theoretical analysis in the first three chapters. The main thrust of this analysis proceeds as follows. He highlights three fundamental features of Olson's original discussion, which Olson offered as explanations of why collective action efforts might fail: (1) the size of the group (the larger the group, the greater the possibility of failure in the optimal provision of the collective good); (2) the composition of the group (the existence of asymmetries among group members in levels of demand or available productive resources can influence the likelihood of collective success); and (3) the institutional framework in which the collective effort is attempted (the existence of selective incentives or informal sanctions can enhance the prospects for collective success). He then assesses the continuing validity of these three features in light of 25 years of additional research on these questions.

This assessment constitutes one of the major contributions of the work. Through the use of recent advances in game theory, Sandler investigates the nature and structure of the strategic interactions involved in collective action efforts. The underlying idea is that the structure of such interactions is much richer and more complex than is presumed by the Olson analysis. By focusing on four central features of the nature of these interactions, Sandler is able to distinguish the types of collective action that are consistent with Olson's original explanation from those which are not. These central features are (1) the nature of the demand for the collective good, (2) the technology of production of the good, (3) any additional constraints on the production or distribution of the good, and (4) the nature of the expectations of the actors (i.e., what one actor believes the other actors plan to do in the interaction). Sandler concludes from this analysis that although Olson's original explanation does not generalize across all of the cases, it remains valid for a wide range of collective action problems.

Readers familiar with the literature on collective action will see that this is not, in and of itself, an original criticism of Olson. Noteworthy about Sandler's effort, however, is the precise and cogent manner in which he presents the many layers of his critique. This analysis is further augmented by his extensive discussion, in chapter 3, of recent theoretical developments related to collective-action questions. The subject areas of this discussion include the theory of clubs, variations on the assumption of strategic rationality, the neutrality theorem for pure public goods, the dynamic aspects of collective action, and the implications of uncertainty. Given Sandler's long interest in questions concerning the theory of clubs, it is understandable that he would devote a considerable amount of the discussion to this issue. But except for an extended discussion of the dynamic questions, the other issues receive merely cursory analysis. More attention to the strategic rationality and uncertainty issues would have been useful, especially to political scientists and other readers of this journal.

The chapters that focus on applications and empirical evidence testify to the power of the rational choice analysis of collective action. In these discussions, Sandler presents an impressive array of substantive examples drawn from economics, international relations, political science, labor relations, trade policy, and classical political theory. In addition to offering a persuasive case for the substantive relevance of the analytical framework that he assesses in the earlier chapters, he investigates the available empirical evidence as to the validity of the basic collective action model. This last discussion should be of special interest to readers seeking empirical tests of rational choice models.

In this book, Sandler has provided an important service to scholars interested in the general problem of collective action among rational, self-interested actors. He has synthesized a large body of theoretical and empirical work in a thoughtful and, for the most part, nontechnical manner. It can serve as a sophisticated guide through a complex and growing literature.

Washington University, St. Louis JACK KNIGHT

A Vindication of Political Virtue: The Political Theory of Mary Wollstonecraft. By Virginia Sapiro. Chicago: University of Chicago Press, 1992. 366p. $53.00 cloth, $16.95 paper.

As one of the very few women close to the political theory canon, Mary Wollstonecraft is a figure to whom feminist scholars repeatedly refer as they work out issues central to the feminist politics of their time. Virginia Sapiro points out, however, that "there is little indication that anyone who played a key role in women's history or feminism, other than Lucretia Mott, read Wollstonecraft's work seriously after her death until the twentieth century" (p. 277). It is only since the 1970s that Wollstonecraft's work has attracted sustained attention and criticism. Sapiro's aim in this book is to defend Wollstonecraft against much of the criticism while contextualizing her work historically and presenting her as a fairly systematic political thinker.

Most of Sapiro's energies are spent depicting a Wollstonecraft who is invulnerable to the charge of (mere) liberalism (p. 279). Against some of Wollstonecraft's feminist critics, Sapiro argues that Wollstonecraft did not support the public–private distinction. Rather, she resisted it, both by writing about family relations in political terms (showing how liberal values like liberty, independence, and consent "have been used against women" [p. 150]) and by insisting that patriarchal domination was no more justified (and no more natural) in the household than it was in the public realm of government (p. 153). According to Sapiro, Wollstonecraft's readers should not be misled by her treatment of the question of rights. Her *Vindication of the Rights of Woman* treated rights as only one part of a complex package that included virtue and duty, as well as other excellences and responsibilities. Moreover, Wollstonecraft valued rights not in themselves or as a means to protect the private realm from public intrusions but as a necessary condition of the development of virtue. Virtue could not thrive in conditions of inequality, degradation, and dependency or in the absence of any possibility of participation in the processes of self-governance (pp. 118, 125). More to the point, no regime could thrive while its children were being reared by women confined in a condition of unfreedom and immaturity (p. 148).

Sapiro covers the whole of Wollstonecraft's corpus: book reviews, essays, letters, political theory, pedagogy, and literature. Proceeding thematically, she draws on all the works together to support her summaries of Wollstonecraft's positions on education, reason, and the emotions; inequalities of rank, property, and gender; and the structures of individual, family, and state relations. The result is a broad-brushed portrait of Wollstonecraft that will be particularly helpful to those who are relatively new to her work. Unfortunately, Sapiro tends to summarize and pinpoint Wollstonecraft's arguments without pausing to assess them, leaving several tensions within Wollstonecraft's thinking noted but unexplored. For example, Sapiro notes that Wollstonecraft appropriates Burke's binary distinction between the sublime and the beautiful only to allow the distinction to collapse: "The meanings of the two words ultimately run together and seem indistinguishable" (p. 200). Yet she investigates no further: Is the problem with the binary or with Wollstonecraft? She rightly highlights Wollstonecraft's use of political language to describe the operations of power in family settings (p. 153), but she fails to assess its effectiveness. Does the political language simply undercut women's traditional roles in the private realm? Does it not also help to refurbish them? Finally, in opposition to those who stress Wollstonecraft's hostility to sexuality, Sapiro suggests a more attenuated position, pointing out that Wollstonecraft "did not confine sexuality to legal marriage" but arguing later that Wollstonecraft's use of the term *married citizen* is no accident, [since] Wollstonecraft regarded family life as a foundation of citizenship for men and women (pp. 144, 174). More problematic still is the fact that (as Sapiro indicates elsewhere) Wollstonecraft actually frowned upon sexuality *within* marriage.

These tensions mark Wollstonecraft's own thinking, and Sapiro is right to record them; but she elides the difficult questions about gender and sexuality to which an exploration of these tensions might lead. For contemporary feminists, Wollstonecraft's views on the status of female sexuality in a patriarchal culture are centrally important. Sapiro notes Wollstonecraft's hostility to, and admiration for, Rousseau and applauds her critical reversal of Rousseau's charge that women are responsible for the destabilizing sexual passions experienced by men. But she does not assess the effects of that reversal, namely, that Wollstonecraft's identification of sexual passion with men divests women of any sexual identity of their own, marking them as the mirrors of male desire. Moreover, the reversal signals Wollstonecraft's tacit acceptance of a more fundamental Rousseauvian insight. She rejects Rousseau's training of women into dependency but agrees with him that sexual energy is a (if not *the*) problem to be solved and that civic virtue, inculcated and maintained by private and public institutions in combination, is the best means by which to do it.

If questions like these are left unasked, it is because Sapiro identifies strongly with her subject. She approaches Wollstonecraft with a strong sense of her worth to twentieth- and twenty-first-century feminists. Wollstonecraft's life and work offer "a wonderful opportunity to think about the process of coming to an understanding of women's lives and gender" (p. 279). Here, Sapiro is absolutely right; and her book contributes to the regeneration of that process in our own time.

Harvard University BONNIE HONIG

Reasoned Freedom: John Locke and Enlightenment. By Peter A. Schouls. Ithaca: Cornell University Press, 1992. 243p. $37.50 cloth, $13.95 paper.

This is the third book by Peter A. Schouls of the University of Alberta on the foundations of Enlightenment rationalism as they are given in the thought of Descartes and Locke. In the first part of *Reasoned Freedom,* Schouls restates and elaborates his point that Locke's idea of general reasoning, as distinct from his particular epistemology, is substantially the same as Descartes's. And since reason proceeds by resolving complex opinions into simple elements and recomposing them into clear and distinct results, it is inherently critical of received complexes of experience and opinion. Schouls thus calls Locke's idea of method "revolutionary," in the sense of "critical." In *The Imposition of Method: A Study of Descartes and Locke* (1980), Schouls had attempted to show, in brief, how the bold individualism of the *Second Treatise* followed from Locke's methodology, a point that he elaborates in two reviews: one of Richard Ashcraft's *Revolutionary Politics and Locke's Two Treatise of Government* and one of Ruth W. Grant's *John Locke's Liberalism* (*Canadian Journal of Philosophy* 19/[1989]). This showing of a connection between the methodology of Locke's *Essay* and the *Second Treatise* is suggestive. But the connection is not as tight or far-reaching as Schouls sometimes implies. There are many individualisms in seventeenth-century political thought and they go with a variety of doctrinal positions on the nature of the state. Schouls, however, has also dealt with the method of reading Scripture used by Locke in his theological essays. And here he is able to show a radicalism that is more clearly doctrinal.

The main portion of *Reasoned Freedom* has to do with the rather different and even larger question of Locke's idea of moral freedom as it is presented in *An Essay Concerning Human Understanding* (esp. chap. 21, "Of the Idea of Power") and as it underlies some central themes in *Some Thoughts Concerning Education* and also *Conduct of the Understanding*. Locke, it is generally agreed, holds that the human agent has freedom to determine its will. We are not completely subject to our natural and immediate appetites but are able to suspend our judgments in order to contemplate the possibility of greater future goods, which, being more remote, do not immediately produce "uneasiness" or impetus to act. But the key issue for Schouls is whether, having recognized these reasonable goods, we are bound to choose them or whether we are always free. Against those commentators who lean toward the first interpretation and claim that Locke, at last, slips into determinism, Schouls vigorously and persuasively argues that Locke remains libertarian on human choice. Locke's teachings, he claims, "are to the effect that human beings are not determined to the greatest good unless they want to be so, and that they are not determined to want to be so determined" (p. 151). Merely to know in what our ultimate good consists is not to choose it. Not to act by reason is no doubt to go against, or to miss, our true interest. But in order to achieve that goal, the human agent must always engage in critical reflection, must always be a "revolutionary" as Schouls would use that term.

This view of Locke, as Schouls acknowledges, is not completely new. It is implied by John W. Yolton in *Locke and the Compass of the Human Understanding* (1970) and is clearly stated by Raymond Polin in "Locke's Conception of Freedom" (in Yolton (ed.), *John Locke: Problems and Perspectives* [1969]). But Schouls has now developed this interpretation critically, worked out all of the details, and traced its influence on Locke's related works. He has thus made a substantial contribution to the literature.

But there is one point on which Schouls's ingenuity may have carried him beyond what the text will bear. I am not sure he needs to find in Locke the idea of a "passion for mastery" or "master passion" that the individual acquires as an instrument of freedom and happiness. Schouls represents this passion as a habit of self-discipline, not from nature but acquired, which is formed by our reason and which, so long as we retain it and keep it active, serves to overcome and control our natural inclinations in the interest of moral freedom and a life of reason. The idea of such a passion seems not to be inconsistent with Locke's general position. But it is not clear that Locke has a concept of this sort. He no doubt speaks of a power to control one's passions, which is greater in some people than in others, and also of a habit, prompted by reason, of suspending choice in order to contemplate remoter goods. But that need not suggest a generalized passion for mastery. The only passion that seems to comes into play for Locke in mastering our inclinations is the desire for happiness and for those proper objects of happiness to which our reason points. Through attentive contemplation and reflection, a desire for those objects may be acquired. But this is not, perhaps, a passion for mastery as such.

But Schouls is surely right in holding that the idea of mastery is an important link between the *Essay* and *Some Thoughts on Education* and that the idea of self-determination is a key to its interpretation. Teaching a child, in gradual stages, to deny its immediate inclinations is the core of Locke's plan of education. But this does not lead, Schouls argues, to the formation, once and for all, of a rational or virtuous character, as many commentators on the *Education* have assumed. The burden of self-determination cannot be laid down. Good character cannot depend on mere habituation, and virtue cannot be engineered. The individual, free to overcome the acquired habits of a bad environment, is also free to lose virtue unless he or she engages in constant self-examination. The ultimate test of the educated mind is thus the practice of rational criticism.

The outcome, then, is a rich and comprehensive account of Locke on reason and moral freedom. Schouls, moreover, is alert to the nuances of Locke's position and to its difficulties, as well as to its strengths. The result is a well-crafted work of scholarship and an illuminating study.

Columbia University JULIAN H. FRANKLIN

The Lockean Theory of Rights. By A. John Simmons. Princeton: Princeton University Press, 1992. 387p. $39.50.

John Simmons's new book is a distinguished example of a style of writing that has established itself within political theory over the past few years. The oeuvre of a canonical thinker or thinkers is presented—often with considerable textual scholarship—as a vehicle for contemporary theorizing. Sometimes the canonical texts are treated primarily as a cue for further reflection, sometimes as a body of ideas that benefits from underpinning in a contemporary idiom (Hampton on Hobbes again), sometimes as a vehicle for understanding a wider movement (Grant on Lockean liberalism), and sometimes for their conceptual possibilities in addressing a whole series of problems (Waldron on private property). Simmons's method is to give a systematic treatment of Lockean rights, aiming both to do justice to Locke's own arguments and to indicate their contemporary significance.

Simmons's argument is threefold: (1) that Locke's (admittedly scattered) remarks on rights can be seen to suggest a pluralistic theory, (2) that Locke's theory anticipated many significant points in contemporary thinking about rights, and (3) that Locke's arguments for natural rights are far more plausible than many scholars have supposed. He draws attention particularly to the right to punish and argues that it (and much besides in Locke's political thought) can be understood in secular terms; for he indicates the Kantian and rule-consequentialist resonances of Locke's views. Locke's account of family, property, charity, and justice are also held to be defensible. Simmons's most general point is that because Locke argued in a plurality of ways, there is scope to both develop and underwrite his views.

This is a most interesting volume, whose scholarship is often admirable (e.g., it is one of the few studies to make good use of Locke's *First Treatise*) and whose implications are worthy of much more attention than a short review can provide. Three comments are worth making, more in a spirit of curiosity than by way of developed criticism. First, while Locke's arguments are certainly intelligible without theological reference, it is less clear that there is not an explanatory framework that will embrace the main features of his political theory. If one were to pursue the implications of Locke's declaration about "the main intention of Nature, which willeth the increase of Mankind, and the continuation of the Species in the highest perfection" (*First Treatise* 59), one might be led to an architecture in Locke's political thought that his different sorts of argument answer to. Second, if that is the case, then the position of rights in Locke's scheme of things is unlikely to be foundational—an assessment that gains force from his statement that if God affords life to people, then "they owe it to his Bounty, they could not claim it as their right, nor does he injure them when he takes it from them" (*Reasonableness of Christianity*. Finally, it is clear that Lockean thought will not accommodate consequentialism in moral reasoning; for Locke's view of toleration (not discussed at length here) precludes this. Locke wished to argue that worship, ceteris paribus, lay outside the ambit of civil government. Now, supposing that the civil authorities have a duty to inculcate truth (a view not so very uncommon till recently and still held in some quarters), consequentialist reasoning would suggest that a judicious use of force might make people attend to arguments that they would overlook otherwise. This is neither an imaginary argument (it was used in Locke's own time) nor an absurd one (Jeremy Waldron has drawn attention to it); but it is an argument that Locke rejected very firmly in his *Second Letter Concerning Toleration*.

None of these remarks is meant to question Simmons's philosophical scholarship or general position. The former has given us a remarkable volume—by far the best treatment of Locke's account of rights and one that is likely to remain so, because it both treats the texts with intelligent scholarship and argues with a high degree of *sophistication* (a combination of qualities far rarer than might be hoped, although required by the subject). It is certainly true that an explanation of natural rights that aspires to a general appeal must be cast in other terms, since theological argument does not appeal to everyone. It is probable, too, that a Lockean view of those rights' content would be acceptable to many. What I have in mind is that the author owes it to himself and to his very important constructive project to establish his view of rights in its own terms.

University of Leicester IAN HARRIS

The Pristine Culture of Capitalism: A Historical Essay on Old Regimes and Modern States. By Ellen Meiksins Wood. New York: Verso Books, 1992. 200p. $59.95 cloth, $16.95 paper.

"Britain may even be the most thoroughly capitalist culture in Europe" (p. 19). This single sentence encapsulates the original, provocative, and often brilliant argument at the heart of Ellen Meiksins Wood's new book.

In advancing this claim, Meiksins Wood challenges a powerful and influential school of thought that portrays Britain as an *incompletely* capitalist society whose legacy of aristocratic agrarianism explains that nation's relative decline over the past century. The latter thesis has been developed most fully in Marxist quarters by Perry Anderson in a series of essays over the past 30 years, recently collected in his *English Questions* (1992). In Anderson's account, an immature English bourgeoisie was able to consolidate its presence within society only by means of an ultimately debilitating alliance with a landed aristocracy wedded to preindustrial traditions. The result was an incomplete modernization of the state (hence the retention of the monarchy) and a culture that favored aristocratic amateurism over bourgeois professionalism. It is here, Anderson maintains, in the incompleteness of capitalist modernization, that we can discover the seeds of later British decline.

Meiksins Wood's critique of this thesis is nothing short of devastating. Ranging freely and with stunning erudition over social and intellectual history, she makes an overwhelming case for the view that Britain represents the most genuinely capitalist nation in the world; and she demonstrates the continuing power of historically informed Marxist theorizing. It is appropriate that *The Pristine Culture of Capitalism* is dedicated to the British Marxist historians (Edward Thompson, Christopher Hill, and the late George Rude, among others); for it bears the stamp of committed socialist scholarship conforming to the highest academic standards.

Like these predecessors, Meiksins Wood locates the origins of British capitalism in the market-oriented agriculture that emerged from the fifteenth century onward under the aegis of a gentry and aristocracy increasingly preoccupied with market prices, wages, and rents. It is in this agrarian setting (where Anderson and others find a retrograde precapitalist ethos) that Meiksins Wood discovers the dynamic impulse toward capitalism.

She argues that in countries like France and Germany, the weakness of organic tendencies toward capitalist development forced the state to intervene to encourage capitalist modernization in an effort to keep pace with Britain's mounting economic, industrial, *and military* might. Britain represents the most purely capitalist culture, therefore, because the state was not a central agency of capitalist development. Since capitalism emerged there organically, private property owners were able to "fence off" their rights vis-a-vis the claims of the state, thus differentiating "economy" and "polity" in classically capitalist fashion.

In a series of brief but remarkably insightful reflections on political theorists like Bodin, Montesquieu, Hobbes, and Locke, Meiksins Wood demonstrates the radically different theorizations of the state that emerge in the French and British contexts. French political theory is state-centered; British political thought, on the other hand, tends to undertheorize the state, focusing, instead, on the social and economic relations of "civil society" (as in classical political economy).

Yet, tour de force that it is, Meiksins Wood's account of the rise of capitalism exhibits an uncertainty about the role of political revolutions. Devoting separate chapters to the no-social-change model of conservative historian J.C.D. Clark and anthropologist Alan Macfarlane, she points out that they treat dramatic and explosive political events such as the English Civil War as if "they need not have happened" (p. 119). But her own treatment seems, at times, to give more ground to such a view than she would like. Criticizing how no-social-change arguments focus on the red herring of whether a single, decisive moment can be identified when feudalism was transformed into capitalism, she writes, "Marxist historians . . . have always insisted on the long duration of the transition from feudalism to capitalism in which revolutions are not the prime movers of change but represent the crises of long and complex historical processes which do not end with the revolutionary episode" (pp. 9–10).

This is unobjectionable in itself, but it begs a key question; for a one-sided emphasis on the "long duration" tends to demote political revolutions to little more than inessential conjunctural disturbances. Short of theorizing the relationship between long-run social and economic changes and concentrated periods of political upheaval and revolution (e.g., Britain in 1640–49, France in 1789–99), Meiksins Wood runs the risk of reinstating the sort of gradualist explanation that she rightly attacks in Clark and Macfarlane. Indeed, her account at points seems to deny these events any central role in the rise of capitalism. Perhaps it is not surprising, then, that the tension in Meiksins Wood's discussion of the relationship between political revolution and capitalist transformation has surfaced in the sharply different positions taken by two of her own students on the question.

George Comninel's *Rethinking the French Revolution* (1987) radically divorces the French Revolution from the rise of capitalism, while Colin Mooers's *The Making of Bourgeois Europe* (1991) argues that the French upheavals and Germany's revolution-from-above under Bismarck must be seen as forms of "bourgeois revolution" brought about as Europe—and then increasingly the rest of the world—was compelled to undergo political transformations in order to meet the challenge of competition for world markets induced by Britain's breakthrough to capitalism.

The Pristine Culture of Capitalism is a landmark study in Marxist discussion of the origins of capitalism. It deserves a further study in which Ellen Meiksins Wood turns her talents to the crucial question that this powerful book leaves unresolved.

York University, Toronto DAVID MCNALLY

AMERICAN POLITICS

Administrative Law in a Global Era. By Alfred C. Aman, Jr. Ithaca: Cornell University Press, 1992. 207p. $28.95.

Shadow Government: The Hidden World of Public Authorities—and How They Control over $1 Trillion of Your Money. By Donald Axelrod. New York: John Wiley and Sons, 1992. 344p. $24.95.

Executive Leadership in the Public Service. Edited by Robert B. Denhardt and William H. Stewart. Tuscaloosa: University of Alabama Press, 1992. 180p. $24.95 paper.

Public administration is arguably the most diverse or ill-defined subdiscipline in political science. (Some, of course, view it as a separate discipline.) The three books reviewed here are illustrative of that diversity in their subject matter. They include public authorities and their challenge to responsible government, executive leadership, and administrative law as a legal and political product. Topics of significance and of interest to various specialists, they are difficult to tie together. Only limited effort will here be given to that task.

In *Shadow Government,* which has some of the qualities of an exposé, Donald Axelrod takes us on a fast-paced journey through the land of public authorities—their development, variety, financing, activities, shortcomings, and accomplishments. Even if one does not share Axelrod's concern or alarm about the wastefulness of many public authorities and their lack of political accountability, one can learn much about them from this well-researched and readable book.

According to Axelrod, there are 35,000 public authorities handling public programs and facilities of various sorts at the state and local levels in the United States. He arrives at this figure by adding several thousand public authorities lacking full financial independence to the more than 29,000 special districts counted in the 1987 Census of Governments. An earlier study by Walsh, *The Public's Business* (1978), reported the existence of approximately 7,000 state, local, and interstate public authorities, which were defined as entities with "independent corporate status, without effective taxing power, and with access to the tax-exempt bond market" (p. 5). This definition excluded most special districts, which possess taxing authority. Had Axelrod followed Walsh's definition, he still would have had an ample number of targets for his study. Indeed, he does not have much to say about special districts per se, many of which have few employees, small debts, and limited scope. His discussion predominately focuses on such entities as the Chicago Transit Authority, Massachusetts Water Resources Authority, Port Authority of New York and New Jersey, Washington Public Power Supply System, New Jersey Turnpike Authority, and New York City's Municipal Assistance Corporation.

As Axelrod amply demonstrates, public authorities often operate with inadequate control by elected public officials and not infrequently in profligate and legally dubious ways. On the other hand, they enable governments to operate many programs and facilities of much public benefit. For example, most mass-transit systems and airports are operated by public authorities and meet with Axelrod's approval. As he states:

> It would . . . be a mistake to play down the solid achievements of many authorities even though the price has been high fiscally and politically. They filled a vacuum left by elected officials who abdicated their responsibilities and produced schools, universities, hospitals, housing, power systems, transportation networks, and water and sewer plants. They dealt with regional problems that respected no political boundaries." (p. 312)

Many of these accomplishments have been done by avoiding restrictions on state and local spending and public debt. Those advocating a national balanced-budget amendment might find the experience of public authorities instructive.

What can be done about public authorities? While Axelrod offers many specific recommendations, he sees the citizenry as confronted with two broad options. One is to continue the hypocrisy of automated and inefficient constitutional provisions, which encourages officials to use public authorities to get things done. The second option is to eliminate spending and debt limits, requirements for voter referenda, and other shackles on government. Citizens would then have to count on responsible, representative government to directly handle the public's business and meet its needs. A sea change in American political culture seems necessary for the second option to prevail.

Executive Leadership is a collection of essays produced in honor of the late Coleman Ransone, Jr., longtime professor of political science at the University of Alabama and an authority on the American governorship. The book's nine essays, which are uneven in quality, deal with the nature of leadership and with executive leadership at the national, state, and local levels in the United States. Variously descriptive, analytical, and prescriptive in content, the essays do not do much to meet Ransone's concern that there was insufficient "reflection" on leadership.

In the opening essay, Charles Goodsell discusses "political professionalism"—which, to some, is an oxymoron. As he defines it, political professionalism is an amalgam of "a competence for power, a capacity for synthesis, an ability to exercise discretion within constraints, and possession of a service ethos" (p. 7). Goodsell says that this concept is applicable to high-level officials, both appointed and elected, because they jointly engage in the same sorts of activities. This thoughtful essay gets the anthology off to a good start, which unfortunately is not maintained. In the next essay, George Frederickson frets about the meaning of "public" in public administration. Recourse to the ancient Greeks and several contemporary authorities does not yield a clear resolution of the matter. Robert B. Denhardt and Kevin Prelgovisk then provide a developmental perspective on public leadership, which is leadership in any situation, governmental or private, that entails mutual determination or direction of collective action. As one might guess, a developmental perspective calls for a touchy-feely definition of leadership, namely, "the character of the relationship between an individual and a group or organization that stimulates or releases some latent energy within the group so that those involved more clearly understand their own needs, interests, and potentialities and begin to work toward their fulfillment" (p. 36). One can imagine Goodsell's political professionals confronting the issue of public authorities, but how would public leaders handle them?

The remainder of the essays focus as various sets of executive leaders—presidents, governors, and city managers. Ryan Barilleaux examines the postmodern presidency. Advice is then offered as to how a new president should act in order to succeed as a postmodern president. In a much different sort of essay, Cheryl Miller and Deil Wright report on longitudinal survey research focused on relationships between governors and state administrative agencies. An interesting finding is that despite "dramatic changes in the institutional strength of the governorship, the perceived influence of that office over state administration as reported by agency heads has not changed appreciably" since the mid-1970s. Students of state government will find this article of much interest.

The topics of other essays include presidential persuasion, federalism and administrative discretion, gubernatorial policy leadership, and professionalism in city management. Each provides useful and sometimes insightful information on the executive leadership process. In toto, *Executive Leadership* comprises a modest addition to the growing body of leadership literature.

Judicial control of administrative agencies and deregulation comes under the lens of Alfred C. Aman, Jr., in *Administrative Law in a Global Era* (which, I suppose, is the time of the global village). As Aman sees it, judicial review of the regulatory process since the early 1930s falls into three eras. The first era, that of the New Deal's Administrative Procedure Act (1933–65), was characterized by broad delegation of authority through procedure-oriented legislation to administrative agencies. Viewed as experts by the judiciary, the agencies were accorded much deference for their decisions. In contrast, the environmental era (1966–80) featured consumer-oriented legislation and judicial activism via the "hard look" doctrine toward agency action. Agencies were expected by the judges to follow often-precise statutory mandates and to provide full and rational explanations for their decisions. Individual rights were given more emphasis.

The third era, that of global deregulation (not a very felicitous designation) began in 1981 with the Reagan administration. Aman argues that since deregulation is largely an executive phenomenon, instituted through statutory interpretation, rather than the adoption of new legislation, the courts have become major deregulatory players. Increasingly, the deregulatory era has been marked by judicial deference to the executive. A substantial portion of the book is devoted to an insightful analysis of judicial action in the 1980s on deregulation and executive power. The concluding chapter on the Montreal Protocol on Substances That Deplete the Ozone Layer seems intended to illustrate the positive side of the global era in regulation and has little to do with judicial review, but it is well worth reading.

Two basic arguments on deregulation are embodied in the book. The first is that "deregulation in the 1980s can be understood as a regulatory response to global competition and part of a federal policy designed to enhance the competitiveness of U.S. Industry" (p. 62). The second, previously touched on, is that "with the exception of congressional deregulation of the Civil Aeronautics Board and a few other deregulatory statutes, deregulation is essentially a program carried out through executive orders," appointments, and other forms of executive action. Aman does not make a convincing case for either contention, nor do I believe that he could. Both are essentially incorrect, finding little support in the literature on deregulation (e.g., Derthick and Quirk's *Politics of Deregulation* [1985] and Weiss and Klass's *Regulatory Reform: What Actually Happened?*). The deregulation of commercial airlines, motor carriers, railroads, intercity buses, petroleum and natural gas prices, and financial institutions (which proved disastrous in the case of savings-and-loan associations) flowed from congressional legislation. A conservative political mood and free-market theory provided context and rationale, respectively.

Administrative deregulation, which was the basic strategy of the Reagan administration, lacks the permanence of legislative deregulation. Deregulation that lives by administrative action can be erased in the same way. Indeed, the volume of agency-generated rules increased during the Bush administration and will likely continue to grow under the Clinton administration, one of whose first actions was to abolish the Council on Competitiveness.

Apart from its highly flawed arguments on the nature and causes of deregulation, a failure to adequately explain such concepts as "regulatory regime" and "regulatory matrix," and excessive references to "discourses" of various sorts, this is an insightful cogent, and systematic analysis of judicial review of the regulatory process. There is much in its pages to mull over and reflect upon; students of the regulatory process will find it a welcome addition to the literature. It is perhaps redundant to say that the book has copious notes.

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Responsive Regulation: Transcending the Deregulation Debate. By Ian Ayres and John Braithwaite. New York: Oxford University Press, 1992. 205p. $39.95.

Clint Eastwood's make-my-day approach to the bad guys, if applied to regulatory enforcement, might provide temporary satisfaction to those wishing to limit the environmental and health harms traced to corporate misconduct. Unfortunately, tougher regulations and stricter enforcement impose extraordinary enforcement and opportunity costs associated with inflexible, legalistic enforcement. Deregulation, on the other hand, reduces intervention costs but leaves unresolved the problems that regulation was meant to resolve. Thus, Ayres and Braithwaite set out to convince liberals and conservatives alike that the critical question is not whether to reregulate or deregulate but how to develop institutions capable of pursuing responsive regulation.

The argument they present is that the current centralized, rule-oriented approach to regulation backed by deterrence-oriented enforcement is cumbersome at best and is particularly counterproductive in the context of confrontational politics and adversarial legalism that typifies U.S. regulatory policies. The responsive regulation that Ayres and Braithwaite seek is based on the ability of regulatory institutions to employ the most suitable approach for a given problem from the "pyramid" of available approaches, which ideally should range from a large base of "laissez-faire" policies to a much smaller set of "industry-wide interventions."

The greatest obstacle to this vision of responsive regulation, they argue, is the need to overcome current adversarial approaches. Even the most traditional regulatory intervention at the apex of their pyramid requires a "benign big gun" that uses prosecutorial discretion to gain the cooperation and good behavior of regulated firms. Cooperative agencies could allow firms to be more flexible in achieving desired regulatory results; and cooperative firms could reduce agency inspection and prosecution costs, allowing the agency to focus its limited resources on the most recalcitrant firms.

Cooperative enforcement and other forms of responsive regulation flounder over the problem of control. To prevent regulatory agencies from being unduly influenced by firms in the ongoing negotiations over acceptable standards of compliance, regulatory advocates attempt to restrict the flexibility of regulatory agencies. Rather than work toward long-term cooperation (and associated social benefits), the weakness of regulatory advocates leads them to seek maximal short-term advantage whenever the temptation arises. In the resultant deterrence outcome, agencies are encouraged to enforce rules legalistically, regardless of cost or benefits of compliance, and firms make every step of monitoring and prosecuting violations as difficult as possible.

Ayres and Braithwaite argue that responsive regulation requires flexibility and trust and that institutionalized participation by those representing beneficiaries of regulation may provide the best chance of replacing current adversarial approaches with more cooperative searches for efficient solutions. The authors consider problems of overzealousness, unresponsiveness, and elitism associated with past participation of public interest groups (PIGs) and discuss in detail the kinds of institutional incentive that would be most conducive to developing a cooperative "tripartite" solution involving PIG representatives, as well as regulatory officials and business in regulatory decision making.

In the tradition of Christopher Stone's *Where the Law Ends* and Bardach and Kagan's *Going by the Book*, the authors seek pragmatic solutions to the limitations of using traditional regulatory approaches and legal enforcement to accomplish regulatory goals. Braithwaite, a sociologist who has written extensively about regulatory agencies in the United States and Australia, teams up with Ayres, a legal scholar whose background in law and economics provides a complementary theoretical framework for the enterprise. The result is a book that combines a broad theoretical argument in favor of hybrid market–regulatory approaches with a pragmatic argument full of examples about feasible programs and realistic problems facing the espoused approaches. For political scientists, this book extends the "new institutionalism's" analysis of alternative solutions to collective problems falling in the spectrum between market and direct government control. Ayres and Braithwaite explore regulatory mechanisms that structure the incentives of regulators, corporations, and public interest groups in ways that overcome current disincentives to achieve cooperative solutions in the presence of specific market failures.

The book is most useful and persuasive in its discussions about the benign big-gun strategy of regulatory enforcement (chap. 2) and about two alternative approaches to regulation: one based on individual firm self-regulation (chap. 4) and one based on the use of discriminatory regulation aimed at particular segments of an industry (chap. 5). The tripartism argument for PIG empowerment is clearly the most controversial and, perhaps inevitably, the least persuasive. The lack of trust between regulatory and deregulatory advocates stands as a roadblock to the very kinds of institutional reforms that would alter payoffs to make cooperative solutions more likely: it takes trust to build trust. The current backdrop of adversarial legalism is not conducive to establishing small pockets of cooperation in specialized niches. Ayres and Braithwaite appear hopeful that dissatisfaction with current regulatory approaches will encourage business and PIGs alike to seek accommodation and experiment with alternative institutional modes of participation. But can experimentation overcome the conditions that gave rise to adversarial approaches to regulation? Or is the cause for conflict buried too deeply in the fragmented system of checks and balances at the heart of the U.S. political system?

State University of New York, Stony Brook JOHN T. SCHOLZ

Conflicting Loyalties: Law and Politics in the Attorney General's Office, 1789–1990. By Nancy V. Baker. Lawrence: University Press of Kansas, 1992. 248p. $25.00.

Independent Justice: The Federal Special Prosecutor in American Politics. By Katy J. Harriger. Lawrence: University Press of Kansas, 1992. 276p. $25.00.

The Solicitor General: The Politics of Law. By Rebecca Mae Salokar. Philadelphia: Temple University Press, 1992. 235p. $39.95.

Sometimes patience, an indispensable virtue in the scholar's enterprise, has rewards unforeseen even for political scientists, who are fortunate if their work can avoid the profession's notorious Bermuda triangle, that indeterminable period between the completion of their research and its publication that so often sucks the intellectual life from whatever were the merits of their original idea. No such fate befalls the authors of these three important books on the complex, reluctant but inseparable relationship between our institutions charged with the development and enforcement of legal rules and the political environment in which the policies that guide this process are made.

No sooner had I agreed to review these three books than I opened the newspaper to find Iran–Contra independent counsel Lawrence Walsh leveling an election-weekend-eve charge against then President Bush suggesting that Bush was indeed "in the loop" on the arms-for-hostages scandal and had misled investigators on his role. President Bush and his supporters would later accuse Walsh of using his position to advance a partisan political agenda in disregard of "the law." Lest the president receive all the attention, just a few blocks east on Pennsylvania Avenue, his attorney general, William P. Barr, was in mortal battle of his own, defending himself against separate charges that he had involved the Justice Department in the Iraqgate cover-up and then lied about it to Congress. Never hesitant to seize the political moment, the Democratic leadership in Congress then demanded that Barr request a federal court to appoint a special prosecutor to investigate these accusations against the Justice Department. That the Democratically-controlled Congress, the nation's preeminent center of risk-and-responsibility aversion, could feel comfortable sending a special prosecutor after Barr and his Justice Department when it had earlier balked at similar requests from President Bush and congressional Republicans to investigate the post office and bank scandals in the House of Representatives, was, one supposes, besides the point.

On such high moral ground has this internecine battle between Congress, the Justice Department, independent counsel Lawrence Walsh, and former President Bush and his supporters been fought. What began, in the Iran–Contra case, as a welcome effort to restore the nation's confidence in its political institutions has evolved into a contemptible war of wills between Walsh, who continues to issue indictments against the former Reagan administration officials that were involved, and Bush, who pardoned them apace. Will the post-Bush electorate, which did not elect Bill Clinton for his position on the special prosecutor's effectiveness or constitutional legitimacy, find the reserve to maintain their interest in a matter than has long since been relegated to sidebar discussion for inside-the-Beltway journalists on their Washington gabfest television shows? Will the nation's collective attention span have the patience to endure a similar point–counterpoint struggle between another faceless court-appointed special prosecutor and a nondescript former attorney general over a scandal that has failed to agitate public opinion? Will the nation ever see the restoration of an executive branch that demands impartial law enforcement and a cooperative Congress not concerned with political invective but dedicated to the confirmation of nonideological individuals of excellence devoted to the rule of law? Will the special prosecutor's existence soon meet the dodo's fate, losing its authorization from a Congress that believes the executive branch has evolved to a point where it can now police itself? Or the Edsel's, becoming extinct through incompetence?

Katy J. Harriger and Nancy V. Baker, in their respective books, *Independent Justice* and *Conflicting Loyalties*, to their credit, avoid the partisan morass that has engulfed serious discussion of the first two questions and focus their scholarship on the latter concerns. Harriger argues that the special prosecutor is neither the panacea to resolve all the ethical and legal missteps of the executive branch nor quite the threat to constitutional government and the separation of powers that the office and the statute that created it have made it out to be. Her work evidences no predisposition to favor the executive over the legislature, independent counsels over those under special investigation, or the standard-issue conservative or liberal dogma that passes for serious discussion on this matter. The professionalism with which she approaches the topic makes Harriger's book the first true comprehensive and scholarly analysis of the constitutional and public policy dimensions of the special prosecutor's office.

Correctly, Harriger does not allow any particular scandal that resulted in the use of a special prosecutor (e.g., Iran–Contra or even Watergate) to control how she pursues the book's two central inquires. Harriger first asks whether the special prosecutor's office is constitutional; if so, then the second concern must be to determine what public good special prosecutor-led investigations have produced that could not otherwise have been accomplished by the attorney general?

On the constitutional question, Harriger agrees with the Supreme Court's 1988 decision in *Morrison* v. *Olson*, which upheld the federal Ethics in Government Act of 1978, the statute that authorized the establishment of an independent counsel's office. Prior to the Ethics in Government Act, special prosecutors had been appointed on an ad hoc basis—a process (as Harriger recounts in fine detail) that had not always worked very well. Confusion over the special prosecutor's scope of powers, constitutional source of authority, and institutional accountability led Congress to pass the 1978 act. Critics of the special prosecutor argued that Congress had no constitutional power to delegate to the courts the authority to appoint special prosecutors. While Harriger agrees with the thrust of the Court's seven-to-one opinion that the independent-counsel law is constitutional, she uses the questions that Justice Scalia raises in his dissent as the basis for her focus on whether the special prosecutor's record of performance matches its promise.

Here, Harriger turns to federal scandals large (Iran–Contra), less large (Wedtech), and now obscure (ethics charges against Carter administration officials Hamilton Jordan and Tim Kraft) and finds the special prosecutor's record a mixed one. While the appointments of special prosecutors have produced some public good, more often than not, their use has failed to accomplish their stated objectives. Special prosecutors, as in the Iran–Contra case, have found themselves caught in a power struggle between Congress and the president. Frustrated, the special prosecutor's office has often pursued its own agenda guided by its own independent sense of justice without any real sense of institutional obligation. In the end, Harriger suggests that frequent resort to an independent prosecutor for reasons that are rooted in political differences between Congress and the president "institutionalizes distrust." This, she concludes, serves no constructive purpose. Rather than authorize the special prosecutor ad infinitum, Harriger suggests that Congress open the statute to reauthorization as such needs arise.

But Harriger also contends that conflicts of interest in the Justice Department are not about to disappear, and warns against the permanent abolition of the independent-counsel law. This compromise solution is not the result of insufficient data to support such a recommendation—quite the opposite. Harriger bases her conclusion on extensive interviews with former special prosecutors, congressional staff members who helped draft the 1978 act, former attorneys general, and several other officials in the Carter and Reagan administrations and meticulous examination of numerous original source documents. We should view the special prosecutor as another of the "auxiliary precautions" that James Madison argued in *Federalist* 51 were necessary to guard the government from itself, but not at the expense of the integrity of the separation of powers. In other words, Trust but verify.

In *Conflicting Loyalties*, Baker turns her attention to the historical difficulties that attorneys general have encountered in their struggle to fulfill what she calls the office's dual role of neutral law enforcer and partisan advocate. A structural tension peculiar to the office—one that, Baker writes, entwines law and politics like no other—makes it impossible to separate the attorney general's twin responsibilities to "the rule of law and rule of man." So what is an attorney general to do when accountable to constituencies whose interests are often at odds with one another? Is it possible for an attorney general to serve as an advocate of the president's interests and still enforce the settled criminal and civil law with which he or she might agree or disagree without losing the confidence of either the president or the public?

Baker's thesis is that such a delicate balancing act is possible and even desirable. No one should expect an attorney general to ignore the policy mandate that comes with the election of a president. But neither should an attorney general allow the Justice Department to become so immersed in the president's political battles or electoral concerns that the office is no longer perceived as one interested in the rule of law. Baker acknowledges that a successful attorney general "may be either an Advocate or a Neutral" but argues that given the special demands placed upon the office, most individuals who assume it will possess some of the characteristics of each. Care must be taken to see that the less desirable qualities of the neutral and advocate officer are tempered.

Fair enough. And Baker, through a selective examination of the performance of attorneys general since the office's creation in 1789, does increase our understanding of what forces push and pull at the office and just how skillful one must be to balance the competing interests at work. Like Harriger, Baker makes good use of original source materials and personal interviews to buttress her thesis. Combined with her comprehensive understanding of the secondary literature, Baker has produced a careful, much needed overview of the attorney general's office and the difficulties its occupants face in trying to satisfy multiple constituencies.

There is some temptation to criticize Baker's neutral/Advocate typology as too simplistic. While she makes a plausible case that Robert Kennedy and Edwin Meese III (advocates) and Griffin Bell (neutral) had different designs for the office, one can argue that the attorney general's posture is as much a function of the president who appointed him as anything else. President Reagan, like Kennedy and Johnson, came to office with a visible and controversial social agenda; President Carter did not. Robert Kennedy and Meese were controversial figures on the front lines of their respective administration's efforts to influence law and public policy, which, in each case, ran counter to established law. Bell, on the other hand, was more content to assume the role of statesman and elder wise man than operate in the eye of the storm, as Kennedy and Meese did. While Bell is quite skillful in portraying himself as a neutral officer of the law in his interviews with Baker, he was not exempt, as she also notes, of "Advocate traits." Bell, like Kennedy and Meese, had been close to the president he served prior to becoming attorney general. What, then, accounts for the typological distinction? Perception, in some cases, might be more important than actual conduct. After all, what is neutral about the process of political appointments and the individuals who staff these offices?

This is a minor quibble with a useful device. Another quibble is Baker's conclusion that Robert Kennedy "may be the century's embodiment of the Advocate attorney general, [because,] to him, politics was a positive force to be utilized to right wrongs [and] could promote the interests of justice." Somewhere lost in Baker's description of Kennedy's pursuit of justice was his consent to wiretap the telephones of Martin Luther King and numerous other high-level leaders in the African-American Civil Rights movement. How such a flagrant disregard for the rights of private citizens escapes Baker's attention is curious, especially given her rightful concern for the danger that excessive advocacy has for the attorney general's credibility as the nation's chief law enforcement officer. But to understand the richness of the office's history and the complexity of its demands, one could not do better than to begin with *Conflicting Loyalties*. Baker has initiated a helpful dialogue on the possibilities and limits of partisan advocacy in an office that demands fidelity to principles sometimes threatened by majoritarian politics.

Rebecca Mae Salokar also confronts head-on another important office caught square in the crossfire of law and politics—the United States solicitor general. In *The Solicitor General: The Politics of Law*, Salokar produces the most comprehensive work thus far available on the executive branch's representative in Supreme Court litigation. The literature, which consists mostly of article-length snapshots on the solicitor general's involvement in, and management of, litigation in the Court, has grown over last decade or so; but Salokar is first scholar to examine the office and its work in such a full-bodied manner over such an extended period. This book, on one level, confirms the consensus that has evolved in the literature, namely, that the solicitor general is the E. F. Hutton of Supreme Court advocates: when the solicitor general participates, either as the principal litigant or as an amicus curiae, the justices listen. From 1959 to 1986 (the period covered in the book), we learn that the solicitor general, in eight classifications of individual rights cases, is the most successful advocate in the Court. While an empirical confirmation of the solicitor general's consistent success within an issue dimension over such an extended period of time is of descriptive interest, if this were the book's sole concern, it would not be news.

What makes Salokar's book so important and (it is fair to write) makes it stand alone in the literature on the solicitor general's role in the litigation process is that she demonstrates through careful, empirical analysis how the office has been used to advance the political and policy objectives of presidential administrations over time. Here, Salokar, without pretense or judgment, demolishes the fiction that Lincoln Caplan passed off as truth in his critical assessment of the Reagan administration's "abuse" of the solicitor general's office (*The Tenth Justice* [1987]). Caplan's central argument—that the solicitor general was, until the Reagan administration made the office an instrument of partisan interests, an officer independent of the temporal politics of the executive branch, sent forth to advocate or defend "the law" without regard to policy objectives of the president—has been discredited elsewhere, most noticeably by Louis Fisher. Salokar does not make Caplan's fallacious thesis the central, or even peripheral, concern of her book. Instead, she allows the data to instruct the reader on the solicitor general's relationship with the partisan interests of the executive branch.

Salokar concludes that the solicitor general's much vaunted independence (which Caplan maintained was an institutional norm until the Reagan administration) is nothing more than a "functional or organizational freedom" from the executive branch. The White House must recognize the solicitor general's expertise in the litigation process and allow the office the discretion it needs to best represent the government's interests in court. But beyond this, Salokar writes, "solicitors general can and do advocate the partisan goals of the executive branch." The solicitor general, no less than the attorney general, commands a position of great power to shape the legal agenda of a president, enabling him to leave a legacy that transcends the whims of the electoral and political processes. Presidents from Johnson to Reagan understood the solicitor general's power to advance their partisan interests through the mobilization of the law. In particular, Ronald Reagan, more than any president since Franklin D. Roosevelt, successfully used litigation (and judicial appointments) to advance the social and political agenda of his administration. This—and not the solicitor general's fall from institutional grace—is what I suspect peeved Caplan and formed the animus for his bizarre thesis on the office's proper function. While I do not count myself among Ronald Reagan or George Bush's foremost admirers, neither would I deny them their day in court. Political jurisprudence, whether spurred by nongovernmental organized interests or the solicitor general, is not, and need not be, the sole province of liberals.

Such concerns do not enter Salokar's scholarship. To the extent that the solicitor general overreaches on the law and asks the Court to go beyond what it is prepared to do, Salokar writes: "The Supreme Court is not without the power to resist potential encroachments by the executive branch. The justices can refuse to hear the government's case or, even more significantly, rule against the United States on the merits. From this perspective, the executive–judicial relationship has the potential for antagonism and conflict." Salokar's insight here is illustrated in the failure of the Bush administration's solicitor general, Kenneth Starr, to persuade the Court during the 1991–92 term to overrule *Roe* v. *Wade*, the landmark abortion-rights case, and arbitrarily to replace a 20-year-old rule applied to alleged violations of the establishment clause with a toothless variation that would allow government much greater power to advance the interests of private religion through public means. Starr, like his predecessor's Charles Fried and Rex Lee in the Reagan administration, counted a number of important victories before a largely sympathetic Court as solicitor general; but that alone will not guarantee success when the institutional interests of the executive and judicial branches cannot be reconciled. On these and other questions about the solicitor general's role in executive branch politics and the litigation process, Salokar's book is now the descriptive and theoretical benchmark from each other work must follow.

Rare is the occasion when scholars are fortunate to publish their work with such timeliness. One hopes that Baker, Harriger, and Salokar will find more to pique their respective interests in these three important offices that hold membership in the political and legal spheres. Their careful and professional but aggressive scholarship deserves a wide audience.

American University GREGG IVERS

Breaking through Bureaucracy: A New Vision for Managing in Government. By Michael Barzelay. Berkeley: University of California Press, 1992. 237p. $35.00 cloth, $14.00 paper.

Barzelay aims *Breaking through Bureaucracy* as a torpedo below the waterline of public administration. Traditional approaches to bureaucracy, he argues, have undermined the effectiveness, efficiency, responsiveness, and political support of public bureaucracy. Despite strong evidence that it no longer works well, the "bureaucratic paradigm" persists. Rules, procedures, authority, and hierarchy dominate when flexibility, quality, empowerment, innovation, service, and value ought to rule. With this book, Barzelay has written a brief for moving past the "bureaucratic paradigm" to a "postbureaucratic perspective" based on customer-driven government.

This argument puts Barzelay squarely in the middle of the lively debate that has been fueled most strongly by Osborne and Gaebler's *Reinventing Government* (1992). Like Osborne and Gaebler, Barzelay does the job with a vigor, even relish, that is likely to irk traditional scholars and practitioners of public administration. In part, this is because, to make his point, he caricatures and oversimplifies the traditional approach: And what image could possibly support a better caricature than bureaucracy? In part, this is because his arguments—especially those about transforming bureaucracy's cornerstones from rules and authority to flexibility and paying attention to the customer—do such violence to the basic precepts of an administrative system governed by law.

Perhaps most of all, this is because Barzelay implicitly confronts the politics–administration dichotomy that has been the central idea of American public administration since Woodrow Wilson. Barzelay seeks to sweep aside, once and for all, public administration's tradition of executing the law while allowing policymakers to define it. The new paradigm he proposes makes policymakers of administrators throughout the bureaucracy by giving them more power, pushing aside traditional bureaucratic boundaries, and enjoining them to be responsive to their customers.

The simple fact, of course, is that bureaucracies are not performing well. The problem that Barzelay identifies is a real one. Moreover, the solutions he advocates have genuine promise: emphasize results, build accountability, expand citizen/customer choice, empower citizens, and deliver value. The critical question that the book must answer is whether the solutions Barzelay advocates will produce the results he contends. The book's evidence comes from a study of administrative reforms imposed on Minnesota state service agencies from 1983 to 1990. Barzelay documents, for example, how managers of the state's personnel and purchasing systems used a new vision of their jobs to revitalize their operations. However, evidence about just how the new vision produced better results, how much improved those results were, and how effectively the innovators could sustain their efforts is thin. The cases tend to identify an unarguable problem, briefly explain how an innovative leader produced new ideas, and then assert simply that the results were better.

Barzelay argues that government has to be more customer-centered. Just what a "customer" is, however, receives scant attention. In a personnel office, for example, "customers" can include, among others, the operating agency that actually hires the candidates whom the personnel office identifies; the applicants themselves; elected executives, who expect not only efficiency but also political responsiveness from the office's behavior; elected legislators, who expect the same but who might have very different views on how it ought to be done;

and the public at large, who demand that a personnel office will pursue certain standards of behavior.

This is not to say that government should not be more customer-driven. Anyone who has waited hours in line for a driver's license knows the answer to that. The point, rather, is that the book confidently argues the value of customer-centered government without probing the concept to discover the contradictions and complexities that give it meaning. Barzelay defines *customers* briefly and a bit tautologically as "individuals or collective bodies—whether internal or external to the organization—to whom employees are accountable as parties to customer relationships" (p. 127). This definition raises lots of troubling problems. In the American democratic system, some customers, such as elected officials, have more legitimacy and more political power than others. Some disadvantaged customers find themselves effectively shut out of the system altogether. Extremely difficult legal and political issues accompany the exercise of administrative power, as the study of public administration itself has demonstrated over the last century. Thinking those questions through is an essential corollary to making government more customer-centered.

The book does not probe thoroughly the problems of defining the customer and of drawing lessons about all of government from a rather limited survey of staff agencies. The empirical and analytical foundations of the book therefore are sometimes a bit thin.

Breaking through Bureaucracy is nevertheless clear and lively. Any scholar interested in examining the performance of American government ought to read it, because it lays out challenging hypotheses. They do, indeed, help break through traditional ways of thinking about bureaucracy. The book's concepts ought to be carefully developed, explored, and tested as part of the continuing search for ways to improve governance in the United States.

University of Wisconsin, Madison DONALD F. KETTL

Revolutionary Sparks: Freedom of Expression in Modern America. By Margaret A. Blanchard. New York: Oxford University Press, 1992. 590p. $49.95.

The civil libertarian history of free speech has a distinguished scholarly heritage. From Leon Whipple and Zechariah Chafee, Jr. to Harry Kalven, Paul Murphy, and Lucas Powe, proponents of broad First Amendment rights have penned distinguished treatises documenting the evolution of expression rights in the United States. Margaret A. Blanchard's *Revolutionary Sparks* continues and broadens—but ultimately perverts—that tradition. Blanchard has many valuable insights on the capacity of Americans to tolerate dissent at different points in the twentieth century. In order to gain the benefit of these insights, however, readers will have to put up with an intellectual arrogance that is unworthy of anyone who purports to defend First Amendment rights.

First Amendment histories have been written almost exclusively by committed civil libertarians. *Revolutionary Sparks* is no exception. Blanchard begins her work by declaring that "fear of ideas and of those who express them, no matter how radical, shows national insecurity rather than national strength" (p. xiii). In the pages that follow, she opposes every effort Americans have made during the past hundred years to limit political dissent and the spread of political information, from the free-speech fights of the early twentieth century to government restrictions on press coverage of the Gulf War. Blanchard is, however, considerably more pessimistic about current affairs than the traditional scholar of civil liberties. Previous commentaries have acclaimed the gradual triumph of toleration in American politics. *Revolutionary Sparks* concludes that as a result of "the repressive nature of the Reagan years," "as the First Amendment celebrates its 200th birthday, it likewise stands in its greatest jeopardy" (pp. 489, 492).

Revolutionary Sparks differs from previous studies in another way. Whereas extant histories of civil liberties are strong on doctrine and weak on history, this work is strong on history and weak on doctrine. Blanchard's historical analysis is far superior to previous work. The coverage of the book is remarkable. Every major twentieth-century free-speech debate is described in detail, and numerous minor incidents are covered in ways that capture the wide variety of political dissenters who suffered official harassment. Blanchard properly recognizes that government may punish dissenters in ways other than silencing speech and delineates all the ways in which elected officials have dealt with hostile critics. Indeed, her study of official behavior demonstrates how the marketplace of ideas may be corrupted by governmental manipulation, as well as state censorship. Finally, Blanchard furthers our understanding of expression rights at different periods of American history by highlighting the criticisms and critics that were routinely tolerated. She correctly points out, for example, that conservative Republicans freely made the same criticisms of United States military policies during World War One that socialists were arrested for uttering.

Revolutionary Sparks is much weaker on legal doctrine. Blanchard has little original to say on these matters, and too much of what she says is wrong. Some of the mistakes are minor, as when the text incorrectly claims that Clarence Darrow argued *Gitlow* v. *New York* (1925) before the Supreme Court. (Walter H. Pollak and Walter Nelles were Gitlow's lawyers.) Other errors are more serious. Blanchard states that Justice Oliver Wendell Holmes, Jr., in *Patterson* v. *Colorado* (1908), "refused to look at the idea that the First Amendment might be applied to the state through the Fourteenth Amendment" (p. 67). In fact, Holmes assumed for purposes of argument that states had a constitutional obligation to protect free speech. The real significance of Holmes's opinion, which Blanchard misses, is his claim that the First Amendment might only forbid prior restraints on speech, a claim Holmes later abandoned.

These mistakes, however, are not the central failing of *Revolutionary Sparks*. Blanchard is not only incapable of understanding how a reasonable person could disagree with her on free speech, she repeatedly denies that any decent person could contest her opinions on any political issue that has been debated during the twentieth century. Virtually every page of *Revolutionary Sparks* is littered with obnoxious editorials on subjects ranging from political economy to foreign policy. Persons labeled "conservatives," in particular, are treated as self-interested moral degenerates. At times, the book seems to lose sight of the First Amendment altogether, as when Blanchard takes a two-page break to criticize Reagan administration policies in Central America (pp. 463–65). *Revolutionary Sparks* consistently notes that "those who would preserve the status quo generally favor quieting dissident speech" (p. vii) but never mentions that most censors also display the same imperious certitude that Blanchard exhibits on every page of *Revolutionary Sparks*.

An extraordinarily important book is buried in *Revolutionary Sparks*. Blanchard presents important information on the changing American marketplace of ideas and expands our conception of how the system of freedom of expression has functioned in modern America. The reader who suffers through her dense writing and contempt for all opinions more than .05 standard deviations from her own will be rewarded with a richer understanding of the American free-speech tradition. Whether anyone not assigned to review this manuscript will suffer for more than 25 pages is a fair question.

University of Maryland, College Park MARK A. GRABER

The Institutional Presidency. By John P. Burke. Baltimore: Johns Hopkins University Press, 1992. 231p. $45.00 cloth, $14.95 paper.

Studies of the presidential staff, according to Burke, fall into two categories: those emphasizing the institutional features of presidential staffing that transcend administrations and those that focus on the different managerial styles and organizational proclivities of individual presidents. Burke skillfully and effectively brings these two approaches together in order to discern how the presidential staff system is amenable to presidential management and "to understand how the institutional presidency forms something of a continuing context, generating predictable problems and effects, within which that presidential management is imparted" (p. xv). This is an interesting and useful extension of the concerns expressed in the author's earlier collaborative work with Greenstein, Berman, and Immerman (*How Presidents Test Reality* [1989]); and it constitutes a fruitful advance in the study of presidential advisory processes.

Burke's general argument is deceptively simple. An "awareness of the organizational context in which a president operates and the relation of that context to staff organization and management is likely to be an important factor in managing, for better or worse, the institutional presidency" (p. 193). The author draws on a range of material and evidence covering the presidencies from Eisenhower to Bush to illustrate the strengths and weaknesses of presidential organization and management of the White House staff. Readers of Burke's previous work will not be surprised to learn that Eisenhower emerges as one model of how presidents should manage the advisory process; but unlike many others in the Eisenhower revisionist school, he also recognizes Kennedy's managerial skills and attentiveness to organizational matters, particularly his ability to correct organizational defects.

While Eisenhower and Kennedy stand as examples of presidents who made different kinds of staffing systems work effectively through deft managerial skills, Johnson, Nixon, Carter and Reagan serve as lessons on how *not* to operate a presidential staff. Indeed, their poor performance in this regard leads Burke to ask whether the organization and procedures of the White House are not so closely bound to the personal predilictions of the president that little change can be expected, making speculations about alternative staff structures and management styles useless. The question whether presidents *can* manage the institutional presidency thus becomes an important focus of the author's conclusions about presidential staffing; but in highlighting this question, Burke is implicitly adding a new dimension to the continuing debate in the presidency literature about whether presidents *ought* to be managers; and by exposing so effectively the inability of recent presidents to control their own staff, Burke does make one wonder about the utility of discussing the broader issue of presidents as managers of the executive branch as a whole.

While Burke is concerned to demonstrate how the organizational dynamics of the presidential staff and the president's management style affect performance and outcome, presidential management style emerges as the more intractable problem because it is so closely (although not totally) intertwined with the personality of the president. The author explores this relationship very carefully toward the end of the book and tries to conclude in an optimistic vein. A president's management style is not necessarily fixed by his personal predilictions, Burke argues, and presidents who are sensitive to organization and management are likely to fare better; but it is a very thin thread on which to hang hope for improvement in the future. Among the last eight presidents, only Eisenhower, with his prepresidential organizational and managerial experience, and Kennedy, whose skill was in learning on the job and adapting, provide any grounds for optimism; and they are the exceptions, not the norm.

Burke's analysis has many implications for those concerned with the nature of the institutional presidency. For a start, it ought to direct some scholarly attention to the neglected questions of how, and why, and under what circumstances, presidents choose members of their White House staffs. Given the apparent incapacity of so many recent presidents to manage the White House, this might well be seen as the point of no return.

Burke also sends an important message to those who continue to indulge in the well-established practice of advising presidents and presidents-to-be about organizational structures in the White House (collegial or formal; chief-of-staff or spokes-in-the-wheel; whether to have economics czars, drug czars, domestic councils, economic councils, etc.) that these things really mean very little in terms of the overwhelming importance of the presidential character itself. The more important issue now is not what structure and organization should be utilized but how presidents can become sensitive to managerial problems when they lack previous management experience or a natural proclivity to recognize organizational and structural defects.

The author also provides an important lesson to presidency researchers about the potential in the presidential archives for substantial research on this aspect of the presidency. Notwithstanding volumes of published work on the presidential staff, there has not been an abundance of hard evidence about how presidents themselves have perceived their organizational and managerial tasks. In this respect, Burke's work is impressive. The Kennedy–Bundy memoranda and the Nixon–Haldeman exchanges prove to be a particularly valuable documentary source and add to our knowledge of those particular presidents.

Finally, Burke focuses on a question that few writers on the institutional presidency have asked explicitly: Does staff organization and management ultimately matter? This, too, ought to trigger more discussion, especially since Burke's own answer to the question is so qualified. It is also a little perplexing, because the author is reluctant to give as positive an answer as the weight of his research would suggest. Moreover, there is a marked contrast between the tone of this conclusion and the one he and Fred Greenstein wrote for *How Presidents Test Reality*, which addresses exactly the same question. Perhaps the reason is that Burke's answer in *The Institutional Presidency* focuses almost exclusively on the relationship between staff work, policy advice, and sound policy choices and tends to overlook the damage that dysfunctional staffing can do to a president's public and professional reputation in a more general way.

There is little to take issue with in Burke's forcefully argued account of the problems of the institutional presidency. He succeeds in his aims admirably, weaving together two important strands of the literature into an analysis that ought to modify, if not change, the direction of future scholarship on the institutional presidency. This book should be read by all presidency specialists, including presidents themselves.

Australian National University JOHN HART

Segregrated Sisterhood: Racism and the Politics of American Feminism. By Nancie Caraway. Knoxville: University of Tennessee Press, 1991. 282p. $42.50 cloth, $18.95 paper.

The title of this book may suggest that its intended audience is solely feminists, specifically American feminists, and that its topical concern is white racism in feminist theory. Such an impression would overlook related themes that animate Caraway's argument. Taking seriously the historical problem of racism in feminism and the tendencies of white feminists to colonize the field of feminist theory, Caraway develops a theory of liberation that draws on black feminist theory, postmodernism, and principles of radical democracy. This theory addresses, as she puts it, "the political conditions for a transformed multicultural feminist polity" (p. 1). The central themes of her argument thus also engage a number of dilemmas and problems that drive political theorists who work in the interstices between critical theory, postmodernism, and feminist theory—how to recast "democracy," "community," and "identity" in light of cleavages of race, ethnicity, sexual preference, gender, and so on and how to rethink "politics" and the "political" in view of a history of political sensibilities that relies discursively on systems of exclusion and hierarchy.

Caraway proposes to join "black feminist theory and postmodernism [to highlight] contradictions and dichotomies invisible within each conceptual scheme" (p. 10). This is a promising strategy. To read the work of black feminist theorists like Audre Lorde, "bell hooks," Patricia Hill Collins, and Deborah King against the grain of postmodernism opens up creative possibilities for rethinking the political. Utilizing black feminist theories to recast the history of feminism and feminist theory, the author creates a "politics of memory" that is first joined to an account of the parallels between black feminist theories and postcolonial discourses and then to an examination of the postmodern proclivities of the former.

Some black feminist theories share with postmodernism a critical awareness of identity as socially and discursively constructed and a suspicion of universal truth claims. In contrast to the decentered subject of postmodernism (which Caraway teaches is antithetical to an activist feminist politics), black feminist theories provide a "fleshiness," a "soulfulness," and a materiality necessary for retrieving the "acid tools" of postmodernism from antihumanism. Moreover, they create a "deep context" to emphasize multiple locations of oppression and to highlight the importance of "lived experience." These elements, Caraway argues, are important in constructing a theory of liberation—a "crossover multicultural feminist theory"—that revitalizes democratic values and renews dialogue.

Caraway's attempt to build a bridge between black feminist theory and postmodernism leads to familiar terrain where multivocality and difference are valorized. The ground Caraway stakes out seems initially intended as a site for the political accounting of difference and multiple identities. Yet it is ultimately marked as a neutral zone of diversity, a location where different experiences can be celebrated, solidarity established, and conflict bounded by the "robust and healing structures of democratic political practices" (p. 196). For all the attention that Caraway directs to the preservation of multiplicity, her theory of crossover politics is fundamentally about the creation of a collective voice periodically spoken by diverse "others" who come together around common issues. The promised more dynamic articulation of difference is lost: missing is a view of the political and politics that insists on the continual evoking of difference, that denaturalizes identity categories such as "black," "white," "woman," and "man" and terms like *experience* not by identifying them as attributes of individuals but by interrogating the discursive and ideological conditions of their constitution.

Such a view of politics would offer a critique of power and its specific operations and suggest that feminism in the plural may signify a critical perspective not a collective singular designation. Moreover, the energy of various women's movements, including the anger that Caraway identifies, could be used to ensure that oppression is not turned back on those in whose name liberation is sought, women. Instead, it could be directed at the multiple locations where power works to create categories of exclusion and otherness. This vision of the political would retain a commitment to black feminist theorists' project of unpacking the workings of multiple oppressions, multiple identities. At the same time, it would claim an allegiance to a postmodern view of theory that, as McClure has observed in "The Issue of Foundations: Scientized Politics, Politicized Science, and Feminist Critical Practice," sees the activity of theorizing as "a contingent and located practice without the security of foundations," rather than the guaranteeing of an "authoritative foundation [for] a unified politics" (in *Feminists Theorize the Political* [1992], 365).

While *Segregrated Sisterhood* falters in its attempt to rethink the political, it does invite attention to the difficulties associated with theorizing "difference," "politics," and "agency" from a postmodernist–feminist location. For example, tracing out the operations of exclusion and erasure, Caraway shows how a white feminist identity is sustained by the suppression of the "other" woman. With this move, an opportunity is created to redefine the category "woman" and to redeploy it for more heterogenous political practices. The points of commonality between her text and this theoretical project suggest the timeliness of Caraway's argument, as well as highlighting her contribution to a number of contemporary debates in political theory.

Arizona State University LISA C. BOWER

Support for the American Welfare State: The Views of Congress and the Public. By Fay Lomax Cook and Edith J. Barrett. New York: Columbia University Press, 1992. 329p. $49.50 cloth, $16.50 paper.

Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States. By Theda Skocpol. Cambridge: Harvard University Press, 1992. 714p. $34.95.

The United States is, it is widely agreed, something of a welfare state laggard. Social policies were inaugurated later here than in many other western countries, they remained smaller, and they suffered more during the attack the welfare state that unfolded in the 1980s. Both of these books attempt to cast light on aspects of this distinctive American welfare state politics.

Fay Cook's and Edith Barrett's useful book examines the contemporary politics of the U.S. welfare state through the lens of public opinion. They interpret the much-declared crisis of the welfare state as a crisis of *legitimacy*. Hence the reality of the crisis can best be explored by means of data on popular and elite attitudes. They report on two surveys undertaken in 1986, one consisting of 1,209 telephone interviews with residents of the United States, the other of 58 personal interviews with members of the House of Representatives. Their analysis is valuable because they make a series of thoughtful distinctions—among programs, the constituencies they serve, degrees of public support, and attitudes correlated with support—instead of merely asking for general approval or disapproval of something called "welfare."

The survey findings are bracing. A majority of respondents wanted either to maintain or to increase benefits in each of the seven social insurance and means-tested programs about which they were queried. Support for particular programs is correlated with assessments of recipient deservingness, with attributions of program effectiveness, as well as with political predispositions and self-interest. Moreover, contrary to what is currently passing for received wisdom, the more firmly supported programs were not necessarily the more "universal." In fact, unemployment insurance, a near-universal program, ranked barely higher than AFDC, the program whose public support is most wobbly. (Although the authors do not speculate about this, at least part of what must be at work here is the resentment provoked by programs associated with the non-aged poor, especially poor blacks and Hispanics.)

Interviews with congressional representatives produced similar although not identical findings. Representatives overwhelmingly favored maintaining or increasing benefits, although deficit-consciousness made them less likely than the general public to favor increases. They were also inclined to rank support of means-tested programs more highly than the programs for the elderly which were favored by the general public.

Cook and Barrett conclude that the welfare state is here to stay. They are no doubt right, and public opinion is an important reason. Still, the very strength of their findings raise other questions. Public support did not prevent a sustained assault on the programs by both federal and state politicians throughout the 1980s, and particularly on the means-tested programs. While Cook and Barrett are correct to say there were fewer cutbacks than might have been anticipated from Ronald Reagan's initial attacks, the unemployment and means-tested programs were nevertheless badly damaged. The point is that while a careful scrutiny of public opinion is instructive, it is clearly only an aspect of the politics of the welfare state.

Theda Skocpol's book sets out a novel interpretation of American social welfare history which casts the United States not as a laggard but as a pioneer. The case rests on the role of 19th century civil war veteran's pensions, and early 20th century programs for women and children (especially "mother's pensions"), in the "origins of social policy in the United States." But the basic logic of this historical argument is dubious. Skocpol thinks the association of civil war pensions with patronage in the minds of reformers spoiled the prospects for social policy initiatives during the Progressive period. But the case is weak. Other patronage-ridden government functions, such as the post office or the schools, were not eliminated, although they were periodically reorganized. Nor were the early programs for women and children important in shaping the social policies eventually inaugurated in the 1930s. The best case that can be made is that mother's pensions influenced the design of AFDC, a very small program compared to the provisions for the aged or the unemployed.

Skocpol's larger aim, however, is not historical but theoretical. It is to provide evidence for a state-centered perspective—now renamed as "polity-centered"—in contrast to theories which emphasize the socio-economic or cultural antecedents of policy. The expansion of Civil War pensions in the late 19th century seems to provide some evidence for this orientation. After all, the pensions were expanded in an era of intense party competition to create a Grand Army of pensioned Republican supporters (mainly among white middle-class native Americans in contested states, raising the question of whether this was in fact a social policy). But while the term "polity-centered" suggests a widening of focus, the analysis actually reveals a rather narrow conception of politics, with reformers on central stage, and patronage the main political issue. In the process, just about everything else going on at the time recedes into insignificance, including the epochal shift in national politics represented by the lopsided election of 1896 and the subsequent Republican domination of national politics. The decline of party competition in turn paved the way for a nationwide campaign by "reformers" to disenfranchise discordant farmer and working class groups, with the result that popular concessions of any kind, patronage-laden or otherwise, became much more difficult to achieve. Moreover, undergirding the Republican victory of 1896 was the work of an alarmed and galvanized business community. Skocpol's focus on reformers slights all of this. And her focus on patronage leads us to ignore other large stakes in social policy, not least of which were business interests in preventing government from shielding workers from employer and market sanctions.

Skocpol's analysis of Progressive era initiatives for women and children poses other problems for her state-centered argument, if only because these were the achievements of a movement that was clearly "society-centered." The middle class women who lobbied for wages and hours protections for working women, for widows' pensions, and for national health clinics for women and babies were obviously animated by a concern for protecting domestic spheres. However remarkable this effort might have been in other ways, it appears to have been an example simply of societally-organized interest group politics. Moreover, as Skocpol herself acknowledges in a book riddled by intellectual reversals, the achievements of the movement were puny: few women got pensions, for example, and the health clinics did not last. The reasons I suspect had to do with power. The federated organizations of the women reformers may have meshed well, as Skocpol says, with the organizational requirements for influence in a federal system. But while this served them in their initial symbolic victories, ultimately their organizations lacked power, and could not sustain the programs in the face of opposition.

A decade ago Skocpol issued a rallying cry, calling for "bringing the state back in." Whatever one thought of those views, they have now been broadened and qualified into the entirely unexceptionable argument that politics matters. But it is difficult to see what is now theoretically distinctive about this work. Nor is this a developed synthesis of the state-centered perspective with the structural functionalist or neo-Marxist theories with which Skocpol earlier did battle. Instead, we are given an amalgam that looks a good deal like pluralism, without however the benefit of a clear delineation of the interest group process that pluralism at its best provided.

Graduate School and University Center, City University of New York FRANCES FOX PIVEN

Political Questions/Judicial Answers: Does the Rule of Law Apply to Foreign Affairs? By Thomas M. Franck. Princeton: Princeton University Press, 1992. 198p. $24.95 cloth, $20.00 paper.

In a book remarkable for its compactness, Franck criticizes the U.S. courts for abdicating their constitutional role in cases involving foreign affairs. Federal courts frequently announce that disputes over war powers and foreign relations are "political questions" to be left to the elected branches. Franck faults the courts on historical and constitutional grounds, pointing for guidance to the more activist orientation of the German Constitutional Court.

Franck is at his strongest in explaining how "abdication crept into the judicial repertory." The political-question doctrine is clearly too abstract to apply in any reasoned manner. As Franck notes, federal judges are constantly juggling "hot-potato" issues like reapportionment, affirmative action, and abortion. Yet those same jurists "tend to turn coy" when they have to decide such issues as the constitutionality of the war in Vietnam (p. 10).

In chapter 4 the author takes us through the standard reasons for judicial abdication: the factual evidence is too difficult, there are no applicable legal standards, too much is at stake, and judges cannot compel the president. Franck does a good job in challenging these conventional justifications. Later in the book, Franck explains why U.S. judges need not be so modest about their role in foreign affairs.

Congress has passed a number of statutes to encourage judicial activity. The 1974 amendments to the Freedom of Information Act authorized judicial review in camera to determine the appropriateness of executive classification of documents. Judges no longer need avert their eyes when the executive branch whispers "secret." The Classified Information Procedures Act of 1980 also brings judges into intimate contact with highly classified information, permitting criminal prosecution in extremely sensitive cases. Finally, the Foreign Intelligence Surveillance Act of 1978 allows domestic wiretaps only after prosecutors obtain authorization from a special panel of federal judges. Clearly, contemporary U.S. courts are positioned to decide cases that might have been beyond their grasp a few decades ago.

Also interesting is the chapter in which Franck explores the history and functioning of the German Constitutional Court, which has no political-question doctrine. Everything is justiciable. Although informative, Franck correctly notes that the German model has limited application to the United States. American foreign relations are "infused with the special circumstances that go with being a superpower." As a result, U.S. courts "must be aware" of America's global role when they are deciding cases.

Less satisfactory is Franck's account for the roots of what he calls the "judicial abdicationist phenomenon" in foreign affairs. He claims that the origins of this phenomenon "can be understood only in terms of the judicial politics of the Supreme Court's earliest years. Marshall's readiness to bargain away some ill-defined degree of judicial review over foreign affairs had been foreshadowed in some earlier political views he expressed while a member of the House of Representatives." Specifically, Marshall said, "The President is the sole organ of the nation in its external relations, and its sole representative with foreign nations" (p. 11).

It is true that Marshall opposed a resolution seeking to impeach President John Adams for interfering with a matter then pending in an American court. But Marshall did not defer because he thought the president wielded a monopoly in foreign affairs or because he thought the courts could not decide matters involving foreign affairs. Marshall supported Adams simply because he was carrying out an extradition treaty between the United States and England. The president did not make foreign policy singlehandedly. Rather, he communicated to other nations our foreign policy *after* it had been decided by the two elected branches, either by statute or by treaty.

In the introduction to chapter 1, Franck quotes Marshall in *Marbury* v. *Madison* (1803) to the effect that the president is invested with certain important political powers that he uses at his own discretion. After referring to the statute creating the Department of Foreign Affairs, Marshall writes, "The acts of such an officer, as an officer, can never be examinable by the courts." This makes it appear that Marshall believed that the whole of foreign affairs constituted a political question for the courts. But Marshall, in *Marbury*, did not say or believe that. Certain executive acts were immune from court review; but when Congress imposes on an executive officer certain duties and "when the rights of individuals are dependent on the performance of those acts," the executive officer is "amendable to the laws for his conduct."

Little v. *Barreme* (1804), decided one year after *Marbury*, illustrates this point quite well. In it, Marshall reviews a proclamation by President John Adams regarding the seizure of vessels sailing to and from French ports and holds that Adams exceeded the statutory authority granted him by Congress. Consequently, the captain who followed Adams's orders was liable in court for the unauthorized seizure. Franck makes no mention of this case, which sheds light not only on Marshall and the early precedents for the political-question doctrine but also on the willingness of American courts to hold the president and executive officers accountable for their actions in foreign affairs.

Congressional Research Service LOUIS FISHER

The Supreme Court and Partisan Realignment: A Macro- and Microlevel Perspective. By John B. Gates. Boulder: Westview, 1992. 253p. $55.00.

The Judicial Response to the New Deal: The U.S. Supreme Court and Economic Regulation, 1934–1936. By Richard A. Maidment. New York: Manchester University Press, 1992. 159p. $59.95.

For scholars of legal processes, the New Deal period was more than a significant event. It was of vast importance to the development of the field of inquiry. Prior to that time, political scientists tended to view the Supreme Court as a strictly legal body, whose members were immune from the pressures of ordinary politics. A vivid example comes from Robert E. Cushman's review (in the *American Political Science Review*) of the 1936 term, one of the most eventful in the Court's history. After the Court struck down a number of New Deal laws in 1935 and 1936 (typically by 5 to 4 or 6 to 3 votes), President Roosevelt retaliated with a plan to appoint one new member for every sitting justice over 70 years old. By 1937, however, the Court itself removed the need for the plan by narrowly upholding recovery legislation—the so-called "switch in time that saved nine." Yet Cushman reports the most tumultuous of periods in the mildest of terms. He begins reasonably enough: "The 1936 term . . . will probably be rated a notable one" (p. 278), then notes Roosevelt's landslide election, the president's reorganization plan, and the Court's validation of New Deal legislation (albeit by split votes). But the final sentence of the first paragraph is jarring: "These facts are spread upon the record as part of the background of the Court's work. No suggestion is made as to what inferences, if any, may be drawn from them." The balance of the article is dedicated to a wholly doctrinal analysis of important constitutional litigation, with no weight given to political or ideological forces.

Despite Cushman's interpretation, we now identify this very period as one that profoundly changed the course of the study of legal decision making. A generation of scholars beginning with C. Herman Pritchett (in the 1940s) through Jeffrey A. Segal and Harold J. Spaeth (1993) spotted what Cushman may have had missed, namely, that justices are not necessarily apolitical decision makers engaged in mechanical jurisprudence. Rather, they come to the Court with well-developed values and attitudes that they work to etch into the law. The early attitudinalists interpreted the deep judicial divisions over New Deal programs to mean that the law was not nearly the guiding light that Cushman and others had claimed. (If the justices were simply following precedent, then why did dissents emerge as a common occurrence?) It was no coincidence that the behavioral revolution's envelopment of the judicial field occurred shortly after the New Deal period.

Legal scholars of a somewhat different inclination also owe their start to the New Deal, for it was the justices' treatment of New Deal programs that renewed important debates over the role of the Court in a democratic society. In his seminal work, Robert A. Dahl (1957) argues that while the Court may have the potential to play a major role by checking the activities of the other institutions, it almost never takes on this function. Rather, the Court usually legitimates the actions of the executive and legislature. And if it does not (as during the New Deal period, when it was full of holdovers from previous Republican administrations), the elected branches often retaliate by proposing legislation to overturn the Court's ruling. Eventually and for various reasons, in Dahl's way of thinking, the Court comes to share the views of the ruling regime, as it did during the latter part of the 1930s. Others, including Jonathan D. Casper (1976), defend quite the opposite perspective, arguing (among other things) that Dahl's study rested heavily on the anomalous New Deal period.

Placed within this context, it is not at all surprising to find yet another generation of scholars who are intrigued with the New Deal period or at least who view it as a useful tool to explore larger theoretical constructs. As its title suggests, Maidment's book centers specifically on the period, using it as a window through which to view judicial decision making. Gates's book, which fits quite squarely with those works seeking to explore the role of the Court in a democratic society, encompasses a longer span of time. It considers not solely the New Deal but four realigning eras defined by critical elections: (1) the Civil War realignment (1837–78), (2) partisan realignment in the 1890s, (3) the New Deal realignment (1911–45), and (4) the critical elections of 1960 and 1964. Despite these distinct foci and significant variation in research approaches and analytic strategies, these works are quite complementary. Both shed new light on complex phenomena and, more important, question whether legal scholars have characterized the New Deal period aptly.

Maidment's work presents a direct challenge to the attitudinal school of judicial decision making. While scholars working within this model (and its early variants) view the 1930s as a turning point, a time when the justices evinced nonconsensual behavior that analysts could explain in left–right terms, Maidment suggests quite the converse. Being fully cognizant of the attitudinal perspective of judicial decision making, he does not hearken back to the days of Cushman; yet he strongly and persuasively argues that ideology was not the only—or even the overriding—explanation of the Court's initial hostility to New Deal legislation. Despite the fact that the justices were often divided over the constitutionality of the programs, Maidment suggests: "They agreed over the broad issues of constitutional interpretation and over the essential nature of the judicial function. They had a common view of the process for evaluating the constitutionality of legislation and used mutually-agreed modes of reasoning and argument in carrying out this evaluation" (p. 142). This process, in Maidment's view, clearly "was not a political decision-making process. It was a legal and judicial process. The judges made their decision within a reference of legal rules and these legal rules provided both guidance and limitation to the exercise of judicial power" (p. 146).

Maidment reaches this startling conclusion about the 1930s by considering the justices' opinions in several state cases (e.g., *Nebbia* v. *New York* 1934) and those directly centered on New Deal legislation. The form of the analysis is an intensive, contextual investigation, which relies on the Court's records, manuscript collections, and some secondary sources. I shall leave it to other readers to determine whether or not his interpretation of the records is sufficiently persuasive. (For what it is worth, I found it sufficiently intriguing to rethink conventional treatments of the period.) But surely, if consensus favors Maidment's position, then the study of judicial decision making from a political perspective (an approach that finds its genesis in the New Deal period) rests on a shaky foundation.

Gates's *The Supreme Court and Partisan Realignment* is a tour de force study of the Court's role in partisan realignments, with its theoretical grounding located primarily in Dahl's study and the work it generated. More specifically, for all four eras (which constitute the major chapter divisions), he tests five expectations flowing from the now-vast literature on the subject:

1. In the years surrounding party polarization and realignment, the critical issues will be found in the cases of state and federal policy invalidation.
2. The salient state policies declared unconstitutional in each period arise from states whose partisan or ideological character is different from the partisan majority on the Supreme Court.
3. The number of salient state and federal policies will significantly increase following a critical election and continue until a majority of the justices are replaced.
4. The Supreme Court will decide salient cases or realigning cases nonunanimously more often than other invalidation cases and more often than all cases decided with opinion.
5. In cases related to the realigning issues before critical elections, the justices' partisan affiliation will be a significant and powerful predictor of their votes in cases raising the salient, or realigning, political questions. (pp. 22–25)

To consider these expectations, Gates amassed a data set consisting of the 743 Supreme Court decisions from 1837 to 1964 declaring federal and state policies unconstitutional, which he coded by issue and by votes. Gates also enlivens his discussion of the data with more historical and contextual analyses of the various periods.

Gates's study and findings are vast and detailed, and all students of the judicial process should read the work. At virtually every turn of the page, I learned something. Nevertheless, several of his conclusions merit particular attention because they either confirm the conventional wisdom or force us to reconsider it. First, and most generally, Gates disputes Dahl's characterization of the Court as merely a reinforcer of majoritarian interests. In particular, his analysis of cases in which the Court struck down state legislation (which, as Casper pointed out, Dahl failed to consider) leads him to conclude: "A wider range of evidence supports a much broader and more complex impact on national policy making. If realignments are the fundamental means of 'tension management,' . . . then Supreme Court policy making is much more important to the course of national politics than portrayed by Dahl's classic study" (p. 168).

Next, and of even greater interest, are his conclusions about the two roles often ascribed to the Court before and after realignments. While some scholars assert that the Court is often "an ally of the majority party before critical elections," Gates finds only mixed support for this proposition: "Before 1860, 1896, and 1964, . . . the Supreme Court handed down several decisions that fueled partisan debate and controversy on the realigning questions" (pp. 174–75). At the same time and contrary to conventional wisdom, the "important role of shaping the majority party's response to the critical issues before realignment is less clear in the years preceding 1932." While Gates finds that the Court "reinforced" the laissez-faire position of the Republican party, "it was not until the clash between the Court and the Democratic party in 1934 and 1935 that partisan passions were aroused." Indeed, Gates reaches a conclusion with which Maidment would surely agree: "In 1934, the Court gave a signal that it might uphold the innovative New Deal measures in *Nebbia*." Another finding concerns the role that some analysts suggest the Court plays after critical elections: it will make policy in conflict with the designs of the new government. Once again, Gates finds mixed support for this proposition, with the notable exception of the New Deal period: "The evidence of conflict following the critical election of 1932 is unequivocal: The Supreme Court struck down thirteen provisions of New Deal legislation and provoked a Supreme Court crisis comparable to the reaction to *Dred Scott*" (p. 174).

The virtues of Maidment's and Gates's works are clear. Both marshal a good deal of evidence that forces us to rethink traditional understandings of the Court. Moreover, the volumes renew our appreciation of the New Deal and its utility (or lack thereof) as a mechanism by which to view legal phenomena. Why they achieved these ends has much to do with their holistic substantive approaches to the Court's docket. Many analysts limit their studies to federal cases, but not Maidment and Gates. Both considered cases in which the Court dealt with state legislation—an approach that adds enormously to the credibility of their stories.

Finally, it is tempting to write that the strengths of one are the weaknesses of the other: Maidment's adept and in-depth treatment of arguments and doctrine is somewhat lacking in Gates's effort; Gates's careful use of data to evaluate his hypotheses is missing in the Maidment study. But such comparisons strike me as unfair to the authors; for both, in their ways, do an extraordinary job in elucidating the phenomena they seek to study. And we, the readers, are treated to a feast of substantive information, not to mention original thinking. I commend both.

Washington University, St. Louis LEE EPSTEIN

Labor's Capital: The Economics and Politics of Private Pensions. By Teresa Ghilarducci. Cambridge: Massachusetts Institute of Technology Press, 1992. 213p. $35.00.

Pension funds constitute one of the most important pillars of the U.S. economy, with pension funds owning two-fifths of all bonds and one-quarter of total equity.

Millions of workers depend on expected pension income to supplement their social security income. The government foregoes as much as fifty billion dollars a year in tax revenue because of the tax-deferred status of pension savings. With pensions having such important impact, one might suppose that scholars would descend like locusts upon the problem of "labor's capital," exploring the logic and contradictions of such an important feature of our current economic system.

No such luck. Pension funds have received surprisingly scant attention among both economists and political scientists. More important, only a handful of scholars have tried to examine the system as a whole, considering its benefits and costs to the full range of actors with important stakes in its operation. Teresa Ghilarducci's important book helps fill much of that analytic void.

Ghilarducci begins by noting that five principal economic agents jockey to influence the operations and disposition of the pension fund system: workers, their unions, firms, financial markets, and the government. None of these actors has perfectly congruent interests, she notes, which means that a fully political economic analysis of the pension fund system must develop a textured, inclusive, complex perspective on the interactions and contradictions among these several actors.

She explores, for example, some of the sources of incomplete pension fund coverage—leaving more than half of the nation's labor force (and roughly three-quarters of women workers) without pension coverage, tracing at least some of this fragmentation to the segmentation of post-World War II labor. She also notes the importance of asymmetric information between employers and workers in the determination of pension insurance benefits and costs. She shows, for example, that workers tend to overestimate the expected value of their pension benefits and that firms willingly offer pension benefits in bargaining, at least in part because they "save on labor costs by providing a benefit workers tend to overvalue" (p. 72). She also shows that employer mismanagement, underfunding, and termination of employee benefit plans grow partially out of firms' growing reliance on pension funds as a source of financial capital; these problems have mounted since the early 1980s as "corporations have adopted sophisticated financial models and tactics to fold pension funds into larger corporate finance strategies" (p. 88).

The book is rich and nuanced in its treatment of this multidimensional political economic system. Its most important scholarly contribution, indeed, is precisely that it seeks to incorporate the views and imperatives of all the respective actors involved in pension funds into a consistent and integrated framework. It is important, further, because it builds from that complex analysis toward a careful and reasoned set of policy recommendations for reforms of the U.S. pension fund system that would potentially address many of its interdependent problems. She argues, for example, that the "employer-based pension system . . . could gradually be eliminated for a nationalized and universal advance-funded system" (p. 177), which would begin to overcome some of the fragmentation, inequities, malfeasance, and heavy administrative costs built into the present system.

This is an interesting, important, and illuminating book. Though the author is an economist and much of her material builds on the economics literature, political scientists should also find it instructive as an exercise in the investigation of both political and economic forces shaping an important U.S. institution.

I find the book disappointing in only one major respect. This book obviously aims to strike a balance between scholarly and more general, especially policy, audiences. In seeking that balance, too much of the scholarly analysis has been sacrificed for the sake of general and policy relevance. I rarely find an academic monograph too short, rather than too long; but I would have preferred *Labor's Capital* to have devoted more space and detail to evidence about the interests and preferences of the respective pension fund actors than it does. For example, Ghilarducci begins the book with a presentation of three competing perspectives about the logic and contradictions of pension funds—neoclassical, Marxian, and institutionalist—but she devotes too little time to sifting through evidence potentially affirming or undermining the distinct hypotheses drawn from these different traditions. While I can understand the imperative to keep the book somewhat more general and accessible, I fear that some of her important arguments will be less than fully persuasive to many of the scholars who are likely to constitute an important core of her readers. This would be a shame, because the arguments are themselves vital and instructive.

New School for Social Research DAVID M. GORDON

Controversies in Minority Voting: The Voting Rights Act in Perspective. Edited by Bernard Grofman and Chandler Davidson. Washington: Brookings Institution, 1992. 376p. $36.95 cloth, $16.95 paper.

Asked in the final days of his presidency what he thought his most important accomplishment, Lyndon Baines Johnson replied it was when he signed the 1965 Voting Rights Act. Few disagree. Indeed, the act may be one of the most important pieces of legislation in this century.

Although the Voting Rights Act's provisions applied to a few political units outside the South, it was aimed primarily at stopping that region's historic practices denying the franchise to African-Americans. Unlike previous legislative attempts to end southern states' wholesale disenfranchisement of their black citizens, the 1965 act worked almost exactly as its designers intended. Whether judged by increasing turnout rates among southern blacks or by a manyfold increase in the number of African-Americans who are elected public officials in the South, or by a newfound sensitivity to minority interests among southern white officials, the Voting Rights Act has generally been regarded as successful.

Controversies in Minority Voting, coedited by Bernard Grofman and Chandler Davidson, grew out of a 1990 conference at the Brookings Institution that provided a twenty-fifth anniversary perspective on the act, focusing on its original goals, its accomplishments, and on what some have come to regard as its "unintended consequences." Particular attention is paid to the 1982 amendments, which critics (e.g., Abigail Thernstrom in her 1987 work, *Whose Votes Count?*) claim transformed the act from an effort to guarantee minorities access to the ballot into an attempt to achieve affirmative action in electoral politics by guaranteeing safe seats for African-Americans and Hispanics. The Appendix contains the provisions of the Voting Rights Act as amended through 1982.

The editors state that they hope the book will "frame and help clarify the debate over the issues of minority voting rights and the concept of effective representation." In the main, their hope is realized. Although no one volume can encompass the myriad of issues surrounding minority voting rights and representation in U.S. politics, *Controversies in Minority Voting* is an important and welcome addition to a sizable and growing corpus. The book covers a broad range of topics, indeed; and a sincere effort has been made to include a variety of viewpoints. Although focusing on any, or a few, of the chapters risks making invidious distinctions, I found the chapters by Davidson, O'Rourke, and Carmines and Huckfeldt to be especially enlightening. The ex-historian in me especially enjoyed Kousser's juxtapositions of the first and second reconstructions' successes and failures. The exchange between Cain and those who commented on his essay is alone worth the price of the book.

Among weaknesses, limitations, or omissions, I, for one, wish greater attention had been given to the 1982 amendments' possible implications for electoral systems in American cities, such as at-large versus district arrangements. One might also wonder whether the book might have benefited by providing Abigail Thernstrom the opportunity to respond to criticisms of her arguments.

Quibbles aside, *Controversies in Minority Voting* is worthwhile reading for all who seek a detailed and balanced understanding of the Voting Rights Act and its accomplishments. Johnson would find nothing here to change his mind about his finest hour.

The book is divided into four parts. Part 1 deals with key provisions of the Voting Rights Act as amended through 1982. Included in part 1 is a chapter by Chandler Davidson detailing the historical background of the struggle for black voting rights in the South and the development of the act itself, as well as chapters by Drew S. Days III on section 5, Laughlin McDonald on the 1982 amendments to section 2, and a more critical assessment of the 1982 amendments by Timothy G. O'Rourke.

Part 2 presents five assessments of the act from several social science perspectives. Edward G. Carmines and Robert Huckfeldt deal with the act's impact on party politics, particularly how it affected the Democratic party inside and outside the South. J. Morgan Kousser uses the Voting Rights Act as the prism through which to contrast the successes of the "second reconstruction" in contrast to the failures of the first reconstruction after the Civil War. Hugh Davis Graham's chapter looks at the act in the context of recent governmental efforts at social regulation. Two chapters, one on expert witness testimony by Bernard Grofman and another on interest groups' efforts at litigation and lobbying, delve into the Voting Rights Act's legal consequences.

Part 3 of the book opens with Bruce Cain's essay exploring the Voting Rights Act's possible consequences for "color-blind" and majoritarian norms in U.S. society. Part 3 also contains commentaries by Luis Fraga, Lani Guinier, Carol Swain, and James Turner on the issues that Cain raises.

Part 4 is an essay by Grofman and Davidson detailing the controversies swirling about voting rights issues and laying out the editors' prescriptions for how the United States might achieve a color-blind society.

University of Cincinnati STEPHEN E. BENNETT

Dirty Politics: Deception, Distraction, and Democracy. By Kathleen Hall Jamieson. New York: Oxford University Press, 1992. 335p. $25.00.

Common Knowledge: News and the Construction of Political Meaning. By W. Russell Neuman, Marion R. Just, and Ann N. Crigler. Chicago: University of Chicago Press, 1992. 172p. $29.95 cloth, $10.95 paper.

These two books represent different perspectives in the ongoing debate about how to define and observe communication and its effects. For several years now, communication scholars have been embroiled in how (and whether) to bridge the gap between the information properties of messages and the information that audiences actually get out of those messages. One point of view has been most concerned with how news and political advertising are produced (e.g., how economic pressures inside news organizations and marketing imperatives in political advertising restrict the range of ideas and limit the logical and factual depth of mass media information). On the other side are those who argue that audiences can engage in a variety of ways with the same message, making the structure and content of information unreliable for inferring what people actually get out of it. These perspectives are easily set at odds by the common tendency of scholars to diminish alternative perspectives in order to make more room for their preferred approaches. These books set examples of what each perspective has to offer, suggesting that both the production and the individual uses of information are important parts of the larger political communication picture.

In *Dirty Politics*, Kathleen Hall Jamieson offers an impressively detailed look at modern political advertising set in a rich historical context. Although she draws upon focus groups to illustrate what people do with the political messages they receive, the real focus of her analysis is on the information structure of the ads themselves. Her attention to the increasingly blurred genres of news and political advertising also alerts us to the logic of complex political messages that use the news to reinforce their advertising appeal. The result is an insightful discussion of the limits of mass communication about the nation's most important political choices. The problem for Jamieson is that although people may draw a range of private meanings from their information environments, the messages themselves are held to low standards of accountability for their accuracy, logic, and relevance.

The detail with which Jamieson analyzes hundreds of ads points in the direction of a grammar of visual and verbal persuasion. The data run from familiar subjects such as the Willie Horton commercials from 1988 and the daisy-girl ad from 1964 to little-explored comparisons with political ads and broadsides from the nineteenth century. Even the familiar subjects are analyzed with impressive attention to detail. For example, her research on the Willie Horton ad reveals that Horton was never known to others, including his victims, as Willie. Willie simply played better rhetorically than William. In an interesting parallel, she described the renaming of Willie Smith, whose celebrated rape trial brought him to national attention as either William or Will. After his acquittal, he returned to being Willie.

In these and dozens of other cases, Jamieson draws our attention to the subtle rhetorical structures of political messages: the use of enthymemes that invite audiences to contribute the unstated assumptions necessary to make sense of incomplete information, the interplay between advertising and news content that turned George Bush into the environmental candidate in 1988, and the visual cues used to associate opponents with enemies and client candidates with voter-friendly images. The visual rhetoric of television permits these moves without the telltale verbal evidence that might prove awkward or embarrassing to those who practice "dirty politics" in this age of plausible deniability.

Jamieson demonstrates that rhetorical analysis need not be based on highly subjective interpretive standards. The construction of political images is a documentable process that begins with understandings about audience information processing. This perspective does not ignore audience reception so much as put the audience in a different place in the model of the communication process. Jamieson looks at citizens less as the end-receivers, and more as the pretesters, of political information. Jamieson argues that far from being defensible as giving people what they want, the emphasis on political marketing does damage to democratic communications by personalizing politics, evading logical thinking, and making the world of political images essentially uncontestable. My only major criticism is that the prescriptions at the end seem particularly weak. There is no discussion of how other nations regulate political advertising to avoid the problems she finds. Instead of thinking about governmental solutions, Jamieson merely echoes the traditional call for citizens to heal themselves—in this case, by demanding better information.

In *Common Knowledge*, Neuman, Just, and Crigler present a strong case for putting the focus of communications analysis on how people use the information they receive (in this case, from the news). Using a variety of survey, interview, and experimental methods, they investigate the responses of 1,300 individuals to five different issues featured prominently in the news. Their approach reminds us that people do not start from scratch when they receive news information but operate from a core of "common knowledge" that guides both their interest in, and their attention to, information. Individual responses to the news are symbolic constructions reflecting unique features of individuals, information media, and issues.

This constructionist perspective begins to show its real potential, about half-way through the book, with several findings that I regard as major contributions to political communications research. Most impressive is a study showing that there is considerable slippage between the dominant media frames for news issues and the framing that individuals use to think about what they have seen in the news. For example, people call upon the "human impact" frame about twice as often as it actually appeared in news about an issue. The frame of individual powerlessness that is common in news reports is used a third less often by individuals in building their personal understandings of those reports. Most interesting is the finding that the conflict frame so common in news reporting is virtually ignored by people in constructing their understandings of issues from the news. The authors conclude that although the news across all media tends to convey a personalized and politically contested world, individuals (at least these subjects recruited from a Massachusetts shopping mall) tend to construct a personalized but depoliticized world from that information.

The constructionist approach also explains why different media do a better job of informing citizens on different issues. For example, people were better informed by television coverage of foreign affairs stories (South Africa and Star Wars), while print sources were more informative for prominent domestic issues like AIDS. The authors conclude that television does a better job of reaching out and personalizing issues that people feel are distant and complex. By contrast, people who are already interested in an issue (like AIDS) become more motivated to extract information from print sources. This suggests new resolutions for the endless debate about which information medium is most informative. Television seems to be the medium of choice for low-interest citizens, particularly those with low cognitive skills, while print news sources better serve those with higher interest and cognitive levels. While this conclusion may not challenge common sense, it opens the door to possibly more sophisticated constructionist hypotheses.

My primary criticism of this book is not with the data or research design, but with the eagerness of the authors to push existing approaches out of the way to make room for their perspective. Some of the claims about the findings seem too bold, and many of the arguments against existing literatures seem forced. For example, I am not convinced by the authors' claims that agenda-setting perspectives on the news have been swept away by their studies. Few scholars in the agenda-setting school claim that the news actually thinks for people. The more common claim is that news frames simply provide a heuristic structure for political thinking. If they accepted this more relaxed (and common) definition of agenda setting, the authors of *Common Knowledge* might have represented their findings as a major advance in the dominant paradigm, rather than as a replacement for it. In addition, their analysis might have been enriched by spending less time distancing themselves from those who study the construction of political messages in the news and recognizing the complementary role of studying how people construct meaning from those messages.

As Jamieson reminds us, there are political and economic factors that drive the production of political information. Without acknowledging this aspect of the communication process, the "common knowledge" that Neuman, Crigler, and Just talk about becomes rootless and devoid of political context. Whence do citizens get their common knowledge but (in part) from the same environment of advertising and news that they are actively reconstructing to suit their personal needs? Much as Jamieson's work might have been strengthened with more attention to how people actually used advertising information, Neuman, Crigler, and Just's contributions might have been enriched with more recognition that the news itself is a political construction. In the final analysis, a broader sense of the political communication process would seem to require understanding both how political messages are constructed and how people, in turn, construct their meanings from them.

University of Washington W. LANCE BENNETT

Institutional Change, Discretion, and the Making of Modern Congress: An Economic Interpretation. By Glenn R. Parker. Ann Arbor: University of Michigan Press, 1992. 118p. $29.95.

In the two decades since the publication of David Mayhew's *The Electoral Connection* (1974), the assumption that incumbent members of Congress seek to maximize their prospects for reelection has become as firmly ensconced in the field of political science as the concomitant assumption in economics that firms maximize profits. Neither are exactly true, of course; but both assumptions are congruent enough with casual observation (and simple enough) to serve as the basic building blocks for quite compelling models.

In *Institutional Change, Discretion, and the Making of Modern Congress,* Glenn R. Parker argues that to best understand the behavior of individual members of Congress and the historical evolution of Congress, it is more revealing to assume, instead, that they seek to maximize the amount of their own discretion. By this view, the considerable effort they undertake to enhance their electoral prospects is really tantamount to the construction of "entry barriers," behind which the full flowering of individual discretion can occur. The need to be reelected is not an end in itself but acts, rather, as a constraint, as might also the whipping efforts of party leaders if they were not so ineffectual. According to Parker, the clearest indication of the increase in individual discretion that has occurred in Congress is the increase in the average number of committee assignments that each member has. Although he does not explicitly say so, this presumably reflects the growth in subcommittee assignments that has been well documented elsewhere.

There is good reason to commend Parker for this undertaking. Models are useful to the extent they lead us to consider questions we otherwise would not, and Parker's does shift some attention to the oft-neglected question of just what it is that members Congress want to be elected for. I think it is also the case that we often think that the electoral connection (i.e., the reelection motive) implies far more about the structure of Congress than it actually does. If we assume that the degree to which members desire reelection has remained relatively constant over the past 30 years or so, it is hard to see how the reelection motive alone can be held directly responsible for the many important structural changes that have occurred in Congress over this time period.

Ultimately, however, I was not persuaded that there is much to be gained by abandoning the assumption of reelection maximization in favor of discretion maximization. For one thing, I was a bit disappointed by the fact that Parker focuses a fairly large amount of attention on the historical growth in "perks" (number of staff, travel allowances, use of franked mail, etc.) that Morris Fiorina analyzed several years ago in *Congress: Keystone of the Washington Establishment* (1977). These are precisely the same phenomena that those arguing for the assumption of reelection maximization have seized upon. In Parker's model, these publicly subsidized assets are viewed, instead, as useful in the construction of entry barriers. Unfortunately, I think little is accomplished in simply recharacterizing efforts at reelection maximization as efforts intended to push out the budget constraint imposed by the need for reelection.

I was also not persuaded that the number of members' committee assignments is a good way to operationalize the key variable of individual member discretion. Parker argues that committee activity is particularly congenial to the exercise of discretion because committees can make "all-or-nothing" proposals to the parent chamber. This strikes me as a rather extreme caricature of the highly "committeecentric" view of the legislature that recent theory and research have tended to undermine (Kenneth Shepsle (1989) labels it the "textbook Congress"). Even if I were a proponent of this hoary old model, I would still need to be persuaded that the raw number of committee assignments matters much. Members have long been willing to cash in several committee and subcommittee slots for a seat on Rules, Ways and Means, or Appropriations. Conversely, slots on the Budget Committee, which can be had without giving up existing committee assignments, are usually not in high demand.

What I see as the most serious shortcoming of this book, however, is Parker's failure to identify just what it is that discretion is meant to be. As he sees it, discretion encompasses an extremely wide variety of behaviors ranging from rent extraction to altruism. Of course, it is true that we should always be on guard against premature closure and must, instead, always be willing to reconsider questions that seemingly have been previously settled. However, when Parker asserts that "there is no reason to believe that vagueness surrounding the meaning of discretion is an obstacle to inquiry" (p. 11), I must respectfully disagree.

California Institute of Technology D. Roderick Kiewiet

The Poverty of American Politics: A Theoretical Interpretation. By H. Mark Roelofs. Philadelphia: Temple University Press, 1992. 271p. $44.95 cloth, $22.95 paper.

Lincoln at Gettysburg: The Words that Remade America. By Garry Wills. New York: Simon & Schuster, 1992. 315p. $23.00.

Reading these two fine books gives tremendous stimulation to one's thinking about the ideas that underlie American politics. They are starkly different in tone. Where Wills is powerfully celebratory, Roelofs is severe and relentlessly pessimistic. It is striking that this should be so, for both authors seem to value the same things: benevolent community over acquisitive individualism, and Biblical traditions over the myth of the social contract.

Wills' book is a tour de force of scholarship, sharply focused on a single event. Placing the Gettysburg Address in the tradition of funeral orations by Pericles and Gorgias, Wills brilliantly argues that Lincoln, whose own martyrdom would soon be added to the liturgy of American democracy, generated in this brief speech nothing less than a transformation in the substance (a good Aristotelian term) of American constitutionalism.

Wills carefully debunks several myths about the speech—that Lincoln prepared casually, that he was upstaged by Edward Everett, that no one at the time understood the significance of what he said. With wonderful narrative skill, he shows how the president seized the pregnant moment to reconstruct the nation's founding principles. This was, he said, a war about the meaning of human freedom. By their valor and sacrifice, "these honored dead" had reclaimed and restored the democratic proposition to which the nation had been dedicated in 1776, reformulated by Lincoln as "government of the people, by the people, for the people."

Roelofs' book is altogether darker. What needs explaining, he argues, are "the conflicts and contradictions that afflict the system generally." His aim is "to establish American political failures not as empirical facts but as theoretical necessities, as design faults" (p. 8).

The problem lies in a schizophrenia in the nation's philosophical foundations. Our dominant *myth,* he writes, is based on the scriptural call to love (Roelofs correctly points out that the imperative of Protestant Christianity is not to work, but to love one another), to build in this new land radically egalitarian, loving communities; but our *ideology,* which Roelofs labels Bourgeois, centers on the liberal commitment to acquisitive individualism. The Protestant strand derives from the Bible and inspired John Winthrop's sermon, "A Model of Christian Charity" (1630), America's "first declaration of independence," as well as Lincoln's oratory. The Bourgeois ideal, writes Roelofs, finds authoritative expression in Jefferson's rational, cosmopolitan Declaration of Independence and in the Constitution.

Roelofs argues that both of these conceptions tend to be privatistic, the former spiritually, the latter materially. Thus, he argues, America has "no indigenous or autonomous language of the public" (p. 42).

Lately, he notes in a final chapter, there has developed a new, "rational/professional perspective" on our political life. However, though its criticisms of the system's performance have been profound, it too has failed to offer a foundation for constructing a more nourishing public life.

A brief review cannot do justice to this rich and subtle book. Roelofs' anguish is apparent, but his analysis full of fresh insights, as he traces to its foundation in America's ideals the culture's ambivalence toward government and toward democracy itself.

His treatment of the major institutions of government is less fresh, less trenchant than his philosophical analysis. It is perhaps affected by the author's sense that "it is much too early by far . . . to start thinking about the 'how' of reform" (p. xiv). In fact he dismisses the current interest in constitutional reform as "almost a joke." The book also ends a little lamely, with a Postscript that briefly addresses the question, how has a system so badly flawed managed to endure for over two hundred years. Partly, he says, it is because it works, and partly because political myths blind Americans to "both the facts and the origins of their sharpest frustrations" (p. 243).

As Alexander Heard once remarked, viability is no mean thing in this vail of tears through which we go. Yet it must not make us complacent. Roelofs shows that the philosophical foundations of our political culture are full of crippling paradoxes, and he is quite persuasive when he argues that we are unlikely to move toward meaningful reform unless we can resolve some of these tensions and begin to employ, in our public discourse, a language richer in common purpose.

One ominous implication of this conclusion is that, should the viability of our institutions come into serious question, we will need a language of public discourse that does not now exist. The Protestant Bible, which served Lincoln so well, has more power in the common culture than many intellectuals know, but its place in the imagination of the elite seems to be eroding. Roelofs' book does point to a fundamental problem.

Smith College DONALD L. ROBINSON

As Far as Republican Principles Will Admit: Essays by Martin Diamond. Edited by William A. Schambra. Washington: American Enterprise Institute Press, 1992. 405p. $34.95.

This book brings together—and makes clear—the two remarkable ways Martin Diamond contributed to the political science profession, broadly understood. First, he played a major role in the reassessment of the nature and ideology of the American founding that began in the 1940s and 1950s with the work of Douglas Adair, Leo Strauss, Stuart Gerry Brown, Perry Miller, and others who taught us again how to take ideas, especially *political* ideas, seriously. Second, Diamond also took seriously—and performed with the utmost distinction—the craft of college teaching in a way that returned the searching Socratic dialogue to the center of the classroom. Though his tragically early death took place nearly two decades ago, this volume reminds us of the continuing relevance and influence of his scholarship and teaching. He was thus not exactly an innovator but, rather, a revitalizer, in most creative ways, of ancient understandings and wisdom that in the presence of modern, "Weberian" social science, urgently needed our attention.

The appearance in 1959, in this *Review,* of Diamond's essay "Democracy and the *Federalist*: A Reconsideration of the Framers' Intent" (the lead essay in the volume here reviewed) was foundational to what became in the 1960s and 1970s, in the writings of Bernard Bailyn, J. G. A. Pocock, Gordon Wood, and others, a new interpretation of the intellectual milieu of the American founding, generally called "civic republicanism." Diamond's argument that the philosophies of the Declaration of Independence and of the Constitution of 1787 were consistent with each other and also in keeping with the most advanced ideas of self-government of the day (termed at the time "republican," rather than "democratic") helped scholars get beyond Progressive historiography preoccupied with the "plutocratic" nature of the Constitution and its alleged betrayal of the American Revolution. As Diamond shows in both the initial 1959 article and in a brilliant series of essays on federalism and *The Federalist* reprinted here, the framers and defenders of the Constitution understood the *real* dangers and dilemmas of democratic government and sought ideological and institutional responses that would nonetheless allow a democratic polity to flourish. In Diamonds own characterization, Madison and the other founders thus were not *anti*democrats but, rather, *wise* democrats; and their rhetorically-more-enthusiastic-but-practically-less-effectual opponents were not so much *super*democrats as *foolish* ones. In making clear this profound yet nuanced understanding, Diamond helped importantly to set historians and political scientists on fruitful paths of inquiry that both clarify our knowledge of the founding era and reveal our continuing need to see why, though democracy may be possible because of human capacity for justice, it is necessary because of human inclination to injustice (to borrow Niebuhr's terms). Diamond was indeed the Publius of his day.

Perhaps even more important, however, as our colleges and universities are convulsed over political correctness and the nature of civil society, is Diamond's attention to "the study of politics in a liberal education," and to "teaching about politics as a vocation." His emphasis, reflecting his own deep Aristotelianism, is on the critical study of the enduring questions of political inquiry: What is human nature? What is justice? and What is the purpose of government?—among others. Only through such study, guided and spurred on by mentors of Diamond's consummate skill (this quality comes through in the dialectic, thought-leading style of these essays) can students achieve genuine intellectual liberation. His critiques of both the overt politicizing of higher education (whether by sixties New Left, or nineties critical-studies, intellectuals), and, even more pointedly, the merely behavioral, value-neutral social scientists that have dominated academia in the last generation or two let us see what is, in Diamond's term, "the highest education." One starts, as Socrates does with Cephalus at the beginning of Plato's *Republic*, with a sympathetic evoking of "what is sensible and healthy in the proposed opinion." Then, through "dialectical ascent" and explanation of "problematic aspects," teacher and student reach "the deeper and universal questions, the questions which transcend any particular polity and which are the universal stuff of an autonomous scientific study of political life" (pp. 305–6). Whether attuned to 350 B.C., 1787, 1968, or 1993, Diamond evokes the essentials of a liberal education and shows us what its political dimensions are, "as far as republican principles will admit."

Syracuse University RALPH KETCHAM

Race, Class, and Culture: A Study in Afro-American Mass Opinion. By Robert C. Smith and Richard Seltzer. Albany: State University of New York Press, 1992. 203p. $44.50 cloth, $14.95 paper.

With *Race, Class, and Culture,* Robert C. Smith and Richard Seltzer make a significant contribution to the debate over the importance of race and class in determining the attitudes and behavior of blacks. They use survey data from the 1980s to lay to rest the notion that as blacks enter the middle class, they become more conservative. In addition, they provide a wealth of information on the nature of current black opinion.

The authors' main goal is to untangle the causes of the differences in the opinions and behavior of blacks and whites. They examine whether these differences are the result of different class distributions, some form of racial distinctiveness, or a combination. They consider the role of culture and group interest in accounting for the racial differences that are not caused by class. A secondary goal is to understand intraracial variations in opinion. They look at the effects of several demographic variables—including sex, age, region, and type of city—and study the influence of religiosity and interpersonal alienation.

Smith and Seltzer compare the responses of blacks and whites to a set of questions about politically relevant opinions and behaviors. They then divide their sample by race and class to see whether the apparent differences between the races disappear when class is controlled. They wisely choose to use several indicators of class: education, occupational prestige, income, and subjective class identification. Using a combination of factors allows them to weigh all of the evidence pertaining to class influence on black attitudes. Many of the studies that have fueled the debate over the role of class in shaping black opinion have relied on a single measure of class. The result has been controversy over the merits of the various measures of class, which has merely furthered disagreements over the relationship between class and black opinion.

The authors have an unfortunate tendency to downplay their most helpful and interesting findings. They do not take full advantage of their measure of upward mobility (their only direct measure of change in class) but include it as an afterthought when looking at the effects of their class indicators. Although they simultaneously control for all of the class indicators as part of their analysis, they relegate their findings to notes or cryptic annotations in their tables. These results could clarify which aspects of class are responsible for variations in the opinions of different classes of blacks and whites. Their book would have made a more significant contribution had they been willing to explore the underlying causes of class differences.

In the preface, they promise that they will refrain from entering the debate on how to define and measure black culture but will assume that racial differences that are not attributable to class are cultural. However, they do attempt to determine whether culture or interest accounts for racial differences that cannot be explained by class location. Rather than establish standards to use in discriminating between culture and interest, they rely on ad hoc assertions based on the secondary literature. Once they characterize a racial tendency as based either on culture or rational interest, they never consider the alternative hypothesis. Some of these arguments are interesting reading, but they are far from compelling.

The authors have gone to pains to make their book accessible to students and lay persons. They provide nice discussions of the problems with their methodology and give a complete example of how they construct their indexes. Most of their tables are clear and succinct. Even so, the book has several flaws that limit its usefulness as a text. In some instances, the discussions of methodology are so unclear—even contradictory—that they are impenetrable. Several times, the tables and the text are at odds, not only because of typographical errors but also because the authors overstate or mischaracterize their results.

Race, Class, and Culture will prove useful reading for those concerned with black mass opinion in general or the effects of class on black opinion. It provides definitive answers to questions about the nature and extent of differences of opinion between middle- and lower-class blacks in the 1980s. However, Smith and Seltzer note that there are reasons to wonder whether more class differences will develop over time. They argue that many new members of the black middle class may not yet be acculturated to middle-class attitudes and behavior. Over time, they may grow to be more like the white middle class. Much of the next generation of the black middle class will be socialized differently, and it is possible that its members will be influenced more strongly by their own interests than the interests of their racial group.

University of California, Berkeley ANN R. KENDRICK

COMPARATIVE POLITICS

British Parliamentary Parties: Policy and Power. By Jack Brand. New York: Oxford University Press, 1992. 370p. $79.00.

In a seminal article published in the inaugural issue of *Legislative Studies Quarterly* in 1976, Anthony King identified several modes of executive–legislative relationships. One that he deemed especially important in the British House of Commons was the intraparty mode. Surprisingly, the relationship between leaders and followers in the House has been much neglected in the literature.

Recognising this gap in the literature, Jack Brand set out to assess the impact of backbenchers in the determination of public policy. The result is a thorough study that does far more than bring Lynskey's 20-year-old attempt to assess the extent of backbench influence in modifying government policy up to date. It goes far beyond it, with an extensive and rigorous volume of research, informed both by a knowledge of theory and of the nuances of British parliamentary life.

Brand recognises that Parliament has several consequences for the political system. The focus of his research is the capacity of a reactive legislature to criticize and modify policy proposals generated by government. The early chapters provide an essential cartography of parties in the House of Commons and a theoretical framework for analysing the impact of backbenchers in the policy cycle. That theoretical framework derives from the literature on policy communities and issue networks, and Brand adopts a sectoral approach in order to tease out the relationship between perceived backbench influence and the presence or otherwise of a policy community or issue network. He chooses six sectors for analysis: agriculture, housing, education, Scottish affairs, economic policy, and defence. These six sectoral analyses (each given a chapter) form the bulk of the book. His conclusion is that even where policy communities exist, there are opportunities for backbench influence and that overall, backbench influence in policymaking is limited but nonetheless significant.

This summary scarcely does justice to a well-informed and complex analysis. The work constitutes a major contribution to knowledge of parliamentary politics in the United Kingdom. There are, however, some limitations to the approach taken by the author. The first concerns methodology. As Brand recognizes and more than once emphasizes, it is almost always difficult to gauge backbench influence (pp. 35, 93, 96). There is the problem of finding out what happened in private meetings; and even if what happened is revealed, there is the impossibility of proving a causal linkage between that activity and a change in policy. Brand's method is to assume that if backbench pressure is followed by a policy change, then that pressure has been important "unless there is evidence that the decisive factor was really the pressure group or a group in the Cabinet or the civil service or some other source" (p. 93). This allows for consistency but has the likely effect of exaggerating backbench influence.

A second problem concerns the choice and number of sectors for analysis. Focusing on sectors is clearly appropriate; but the result is that one learns as much, if not more, about policy in a particular sector than about the influence of backbench members of Parliament in that sector. The selection of as many as six policy sectors also means that treatment of particular instances of backbench pressure, even when apparently having an important impact on outcomes, is often breathless and fails to allow the author to draw out the nuances—and, indeed, the significance—of the occasion. In 1984, for example, the proposal by the education secretary Sir Keith Joseph to increase parental contributions to student grants was met by massive resistance on the backbenches. There was extensive lobbying of members of Parliament by parents, especially at constituency level. It looked as though a majority of the parliamentary party would actually vote against the government if it persisted with the proposal, and the proposal was dropped. Yet none of this is really brought out in the few lines devoted to it in this volume. Likewise with the resignation of Edwina Currie as a junior minister. We are told that "the discontent of her back-bench colleagues played a large part in this" (p. 139), but consideration is confined to a short paragraph.

Finally, I spotted about a dozen misspellings of names, compounded by spellings in the text not always coinciding with spellings in the index. Although this does not detract from the quality of the work, it does constitute a slight irritant.

None of this should detract from the overall impact and importance of this clear and very readable book. It is the product of extensive and thorough research. The result is a major contribution to the study of Parliament in the United Kingdom, serving, as it does, as a corrective to many current assumptions about the consequences of Parliament. It fills a notable gap in the literature and will constitute the principal work on the subject for many years to come.

University of Hull PHILIP NORTON

Generals in the Palacio: The Military in Modern Mexico. By Roderic Ai Camp. New York: Oxford University Press, 1992. 278p. $49.95.

This is probably the most up-to-date and analytical, but mainly sociological, study of Mexican generals and politics that we have today. The author's research and methodology are impressively comprehensive, as are his efforts to analyze the role of the Mexican military in recent times. The book amounts to the latest, and probably the only, structural analysis of the Mexican military's attitude, nature, structure, role, and image vis-a-vis the civilian sector, as well as the civilian perception of the military. It is obvious that Ai Camp has researched the literature extensively. As a result, the book ends up as a highly useful bibliographical contribution to the study of the Mexican military and Mexican politics, a subject that Ai Camp has also written on.

Aware of the extensive literature that exists on civilian–military relations, Ai Camp has concentrated on several areas: the army emergence from civilian origins, the Mexican revolutionary environment, the military's restraint in refraining from overt political activity, its professionalism, the civil–military interlock, and the military's class origins and education. The result is a book that amounts to an important survey and a considerable empirical presentation locked into a Janowitzian sociological methodology, which, in the end, prevents it from becoming a classic work. It is a book that tells us more about the sociology of the Mexican military than the role of the Mexican *military in politics.*

Only near the end of the book does Ai Camp come to the core that might have made the work even more substantial than it is, with the sections "Military Civilian Interlock" and "What Kind of Relationship?" This should have been the book's centerpiece if the author's intention was, indeed, to examine "generals in the Palacio." In fact, any author writing about the Mexican military should start with the question, "Is it possible to study Mexican political history without a study of the military?" After all, it is quite possible to write a political history of Great Britain, the United States, Denmark, or West Germany without considering the military, except for a glancing encounter with NATO. Not so for Mexico, which is a Third World country.

Thus, structural analysis of the Mexican military deprives us of the fundamental understanding of Mexican politics, a field in which, as the author himself admits, "the lack of attention paid to the Mexican military is remarkable." Why is that? Because, Ai Camp contends, Mexico is "a Third World society that has successfully limited the military's political involvement." Why, if, as Ai Camp says, the Mexican case established a "longer reign of civilian supremacy than in any other country in Latin America," is the book still called *Generals in the Palacio*?

The American military, like the Mexican, also participates in joint-government disaster relief projects at home and abroad and in national security debates. It does not, however, participate in politics, campaigns, or elections. But in Mexico, campaigns, elections, and opposition parties form, for the military, "the most obvious means of political involvement." Then it follows that perhaps the real subject of the book should be the extraordinary role played by the Mexican military in officeholding and electoral and political activity. And why should this case be so different for Argentina, Brazil, Chile ("ABC") or other Latin American states? The only apparent reason is that Mexico appears more stable, less praetorian—a political society where the military and civilian authorities accommodate each other.

The structural and sociological models that Ai Camp scrutinizes and examines so closely obscure the importance of the real *political,* not sociological, phenomena—that is, the generals' presence in the Palacio without benefit of, or resort to, coups. What distinguishes Mexico from the Latin American ABC cases, according to the author, is its "broader societal interactions," which create a balance between the military and society and thus political stability. Still, what remains—and what is ominously important—is a condition in which the military *could* upset the balance, because of its corporate tendencies and orientations.

What, then, is the use of the "equilibrium model" if the equilibrium is vulnerable to praetorian opportunities, which, luckily, Mexico has managed to escape so far? This brings us back to the basic question: What are the politics of Mexico without the generals in the Palacio? Thus, the "interaction between professionals and important political actors" can only be explained by the political and institutional arrangements of Mexico, not by the structural arrangements of the military and the civilians. Surely, a symbiotic civil–military linkage in the Palacio is not the safest guarantee for political supremacy.

The author ends the book with a profound statement that seems to make the sociological-equilibrium model entirely irrelevant: "The increasing role of the military, for a variety of the aforementioned reasons, primarily focusing on internal security, legitimizes its potential for a larger voice, not just in the *execution* of policy but in the *formulation* of policy. In a society where military political intervention is seen negatively by the majority of citizens, a margin of error exists. It is unknown how widespread such thinking must be before military intervention is no longer viable as a political alternative in Mexico" (p. 230).

Thus, when we speak of "civilian control," what sort of civilian regime do we have in mind: democratic, totalitarian, or corporate? All have a tendency toward dominating the military. All govern through the primacy of their respective systems. It is true that a corporatist Mexico guarantees "civilian" control. But in such a system, which is bureaucratic and hierarchical, why cannot the military find a home, as they have in similar systems in Latin America.

It is clear that when it comes to explaining civil–military relations, the Janowitzian approach is inadequate, at least when it comes to Third World countries of Latin America. It is equally clear that the role of the Mexican military in politics, however subtle, is also crucial. The symbiotic relationship exists and is deeply rooted. By their very presence in the Palacio, the generals are breathing down the necks of the civilian presidents. Unfortunately, this situation and state of affairs is not directly presented or forcibly argued by Ai Camp in a book rife with opportunities not completely taken. Through his extensive and painstaking research, Ai Camp could have written a landmark book on the role of the military in Mexican politics, which appears to have been his ambition. Though certainly outstanding, it is less than that: the book is a diamond, but a diamond in the rough.

American University AMOS PERLMUTTER

Power in Africa: An Essay in Political Interpretation. By Patrick Chabal. New York: St. Martin's Press, 1992. 311p. $49.95.

Patrick Chabal, Reader in History and Politics of Africa at King's College, University of London, states at the outset that it is "an old fashioned book" aimed at stimulating a "debate about the interpretation of politics in post-colonial Africa. His basic assumption is that today Africa is in crisis; it suffers from astronomical debt problems, corruption is widespread, development has been stifled and that a large number of people suffer as a consequence. He correctly believes that for too long, analysts have focused on the uniqueness of Africa rather than understanding the process as we do anywhere else in the world. In order to address this problem, Chabal develops five concepts which he maintains stand at the core of his political analysis: the political community, political accountability, the state, civil society and production. The continuities and ruptures between precolonial, colonial and post-colonial communities provide the material from which it is possible to understand the contemporary nation-state. "Our failure to make sense of some of the most momentous (if not tragic) events taking place today in Africa can often be traced to our lack of understanding of the notion(s) of communities held by the various actors involved in those events" (p. 53). For Chabal the absence of political accountability revolves around the fact that before independence both bureaucrats and politicians had to operate according to principles of political accountability in which there was an ultimate sanction enforced by the colonial rulers and that this is now no longer the case. After independence, the principles of accountability had to be reinvented and in the process pre-colonial, colonial and nationalist pasts came into conflict. The colonial state made the law, its bureaucracy implemented it, adjudicated, passed judgement and carried out sentences. Independence changed the notion of state legitimacy, as well as the relationship between the individual and the state. "It is the drive for hegemony, the political contest for supremacy between the state and civil society, which is the hallmark of contemporary African politics" (p. 81).

Chabal proceeds from this point to use his five concepts to reinterpret major crises facing contemporary African countries such as nationality and sovereignty, legitimacy and representation, accumulation and representation, good government and public morality, and violence. The nationalist state was "condemned" (p. 123) to invent a nationality from above; it had to create an effective nationalist party and cement national unity. The nationalists came together only as Africans, a category clear enough in racial terms but otherwise extremely loose and vague. In any event, for Chabal the strong, centralizing, homogenizing, coercive and politically supreme state is under attack throughout the world. He sees civil society fighting back against the state-centered model of society and in Europe the new political agenda stresses decentralization, regionalization and individualization. "The same is likely to happen in Africa regardless of the state's integrative discourse" (p. 135).

In order to understand the difficulties of legitimacy and representation Chabal maintains that we need to analyze how apparently non-political activities fulfil political roles, i.e., kinship, family, and other forms of interpersonal relations (p. 149). The essence of Chabal's argument in terms of the accumulation and representation crisis is that in many African states the quest for power is the highest economic objective. Since the degree of control of state power is the most accurate indicator of economic success, it is not to be expected that accumulation and inequality will necessarily be productive. Because of a combination of historical, economic, political and institutional reasons good government has been rare and this has undermined the legitimacy of post-colonial states as well as their capacity for purposeful action and has brought about a crisis in political morality. The 1970s and 1980s have seen Africa depicted as a continent of violence, but it is not simply a matter of the violence of repression, of which there was an equal amount in many other parts of the world, but the violence of neglect and unintended consequences and in many cases (except in those cases where bad government has reduced the whole political edifice to rubble), progress has been made towards the creation a political order to suit a new Africa.

In the final section of the book Chabal considers the dynamics of what he calls political Africanization. In Africa the notion of the individual *ipso facto* contains the communal and in contemporary Africa, as in many other parts of the world, "the legitimacy of power derives in part from personal wealth and that, conversely, it is legitimate to use power as a means of amassing wealth" (p. 216). Furthermore, the state has survived as state only insofar as it has been successfully colonized by civil society; the state and civil society are silent accomplices in the maintenance of a political order which is productive for both. "Political analysis must thus seek to move beyond the examination of the way in which state and civil society publicly joust and focus instead on the shadowy world of politics in which the deals between the two are struck" (p. 232). Chabal recognizes that the effects of dependence/aid reinforce the state economically and politically and boost its power over the power of civil society. He concludes with thoughts on the important notion of the reproduction of power: ruling elites have been more concerned "to accumulate than to invest" (p. 260) and hence the reproduction of power in contemporary Africa is still uncertain.

Power in Africa is a sweeping interpretation of politics in contemporary Africa. Patrick Chabal asks the right questions and points the way to an improved understanding of political, social and economic complexities. The book is far removed from current micro-studies and this is its strength and weakness. While his analytical framework is based on universal political concepts, in applying them Chabal ultimately emphasizes uniquely African predicaments and responses. Nonetheless, this is an important and provocative study which is of relevance to those concerned with comparative politics, political development and African politics.

Indiana University PATRICK O'MEARA

Reform and Transformation in Eastern Europe: Soviet-type Economics on the Threshold of Change. Edited by Janos Matyas Hovacs and Marton Tardos. New York: Routledge, Chapman & Hall, 1992. 345p. $79.95.

Dismantling Communism: Common Causes and Regional Variations. Edited by Gilbert Rozman. Baltimore: Johns Hopkins University Press, 1992. 405p. $38.00.

Both of these books address, directly or indirectly, the central event of our times—the transition from communist rule in the former Soviet Union and East Central Europe. There has been no shortage in the discussion so far of rival versions of "who got it right." There has been some consideration of the future of Sovietology and communist studies and whether they have a future at all. There has been much less interest, so far, in the process of transition itself—in the relative weight of political and economic factors, the influence of broad societal forces as against the actions of political elites, and the relevance of transitions from authoritarianism in other contexts. Both of these studies—Rozman's, particularly—are among the first to view the transition from the vantage point of the collapse of communist rule in its country of origin in the early 1990s. Neither, however, is entirely satisfactory.

Gilbert Rozman's edited volume is the better of the two, and it has several clear virtues. One of them is an explicitly comparative approach, particularly in the editor's own chapter on stages of reform. These stages, it emerges, are five: (1) leadership, as selected through a competitive process; (2) public sentiment, or the wish to abandon communism; (3) modernization, or the pressure for more radical economic reform than the leaderships originally intended; (4) integration into the world environment; and (5) ideology, or the abandonment of Marxism-Leninism. But is this a theory? Are these stages logically related, and do they form a necessary sequence? They may fit the Soviet Union and China—but what about deviant cases such as Bulgaria, where the former communists won competitive elections in 1990, or, more recently, Lithuania, where the former communists were voted back into power?

These difficulties are compounded by the remainder of the volume, which is loosely constructed even by the standards of conference volumes. The book evidently took shape between 1989 and 1991 (and therefore before the collapse of the Soviet Union at the end of that year); yet some of the chapters adopt the viewpoint of mid-winter 1992. There is a certain amount of repetition from chapter to chapter. Some chapters (like Tsuyoshi Hasegawa's) advance typologies of their own that other chapters do not consider. And there are unresolved disagreements among the contributors—in particular, about the long-term viability of the "Chinese model" of economic liberalization combined with political authoritarianism.

Perhaps most frustratingly of all, there is no serious, extended coverage of the key variables that are involved in the process of transition. There is no proper discussion of economic performance, whether long-term growth rates or the availability of consumer goods. Yet there seems little doubt that some form of socialism would have survived, in the USSR at least, if economic growth had not ceased and then moved into reverse at the end of the 1980s. There is no chapter that focuses directly on public opinion, including attitudes toward political reform and the market and perceptions of regime performance. There is more (but still not enough) about the governing elites: How united were they, and how had their composition changed during the late communist years? And there is less about East Central Europe, particularly the Balkans, than about the two communist giants.

There are nevertheless some good things in both of these volumes. Rozman's volume brings together specialists on the Southeast Asian regimes with experts on the more familiar systems in the former Soviet Union and China; and several other chapters are explicitly comparative in a manner that may lend itself to application in other contexts. Hovacs and Tardos's collection, though it has relatively little to say to political scientists, still contains some acute observations, such as Alec Nove's on the shortcomings of neoclassical economics and Robert Davies's on the development of a Soviet debate on models of economic reform. This collection is also unusual in including the views of several academic economists now in government in East Central Europe.

My favorite is a chapter by Takayuki Ito in the Rozman volume on political legitimacy in Eastern Europe. Ito builds upon both the classics and more recent scholarship, and he reaches somewhat unconventional conclusions. Communist systems, for a start, cannot readily be classified in Weberian terms. Again, legitimacy is just one among a number of factors that generate support for a communist system. In Bulgaria and Romania, former communists enjoyed some electoral success. And survey evidence, at least for Poland, suggests that the regime "rested on a foundation, albeit a shaky one" in the mid-1980s. How was this foundation generated? To some extent, Ito argues, through an appeal to a millenial ideology and, perhaps to a greater extent, through rapid industrialization and economic performance. Ito's discussion relates very largely to Poland; but if other chapters had adopted a similar methodology, the result would have been a substantial contribution to the theorizing of a process of political transition instead of a series of useful preliminary sketches.

University of Glasgow STEPHEN WHITE

Letting the People Decide: Dynamics of a Canadian Election. By Richard Johnston, Andre Blais, Henry E. Brady, and Jean Crete. Stanford: Stanford University Press, 1992. 316p. $37.50 cloth, $12.95 paper.

This book about an election opens like a suspense novel: "On July 20, 1988, John Turner . . . took the biggest gamble of his career." Will the gamble pay off or ruin the gambler? Most of us probably do not follow Canadian politics enough to know the ending right away. But even if we do, the plot is tantalizing. We are eager to learn what calculations motivated the gamble, what cards the other players in the game held and how they played them.

This opening sets the tone for a book that is both enlightening and captivating. The setting is the 1988 Canadian election. Like few other ones, this one is being waged over essentially one issue, the free-trade agreement (FTA) with the United States. Free trade, like free silver at some earlier time elsewhere, evokes strong feelings in Canada. Moreover, it divides the public along some well marked lines. This is an electoral fault line of considerable historical depth. But it is not automatic that free trade must be a fearsome issue in the particular election of 1988.

Whether or not it will be depends, as Johnston and his colleagues forcefully argue, on the strategies of political parties in pursuit of electoral support. It was the decision of the leader of the Liberals at the time, John Turner, that "forced the election on the [free-trade] issue because he thought that over one hundred years of history would weigh heavily against the Conservatives." He wanted to "let the people decide" this historic question, hoping no doubt that the people would back his side (opposing the free-trade agreement). On election day, however, Canadians failed to do so, instead handing victory to the governing Conservatives, who had negotiated the agreement, but had not made it their issue of choice for the campaign. In other words, Turner gambled and lost.

Johnston and his colleagues undertake a painstaking analysis of how this happened. They chart voter opinions on the free-trade issue and party preferences throughout the campaign, documenting the sharp ebb and flow of support for the major contenders. All the early signs pointed toward success for the strategy of the Liberal Party, with a televised debate making a major impression on the electorate. Certainly, here is a campaign that had more than minimal effects.

It is a masterly analysis, crafted with a sure knack for not letting technical points overwhelm the flow of the discussion. The study aims higher than to simply unravel the mysteries of one election. The major point of theoretical interest concerns the "priming effect." As Johnston, et al. see it, what explains electoral outcomes is not so much some underlying division of partisanship, or the positioning of parties in an ideological space or in social cleavages, but the choice of a particular issue. "To prime is to choose," as they put it most succinctly. The choice of free-trade "allowed [Turner] to prime, to arouse, basic sentiments among Liberal party members and to give those members reasons for voting for him."

The authors argue their case very persuasively. But knowing how the election turned out, one wonders if the case for priming is so compelling. It is not so much that the gamble did not pay off. It is that its payoff appeared dubious from the start. The Liberal leader chose an issue that did not promise victory. Regardless of what opinion poll one trusts, there was no majority in the Canadian electorate for the Liberal position on this issue. It is more surprising that this party surged for a while than that it lost in the end. Most of the media effects prove ultimately unhelpful for Turner's party. If this were a financial report, the bottom line would not impress many stockholders. If anything, the Liberals' priming of the free-trade issue may have inadvertently helped the competition, which had achieved something that was not unpopular.

Even though the suspense about the story lingers and some doubts about priming remain, this book is a must read for anyone in electoral research, not just Canadian; it is also very rewarding and thought provoking.

State University of New York,
Stony Brook
HELMUT NORPOTH

The Collapse of State Socialism: The Case of Poland. By Bartlomiej Kaminski. Princeton: Princeton University Press, 1991. 264p. $39.50 cloth, $14.95 paper.

The Roots of Solidarity: A Political Sociology of Poland's Working Class Democratization. By Roman Laba. Princeton: Princeton University Press, 1991. 247p. $24.95.

The Dynamics of the Breakthrough in Eastern Europe: The Polish Experience. By Jadwiga Staniszkis. Berkeley: University of California Press, 1991. 303p. $35.00.

For several reasons, the collapse of communism and the recent transitions in Poland have received a great deal of scholarly attention in the last few years. The post–World War II political history of the country is replete with social movements, political crises, and labor unrests—extraordinary events in the usually stale fare of Soviet-bloc politics. Owing to the protracted nature of the breakdown of Polish socialism, to the fact that Poles were the first to succeed in repelling communism, and to a number of other unique factors, the country offers a case with abundant and stimulating research opportunities other states in the region can scarcely replicate. Furthermore, the number of scholars working on Poland exceeds the pool of specialists devoted to the study of other East European states—perhaps not least because Poland itself has produced many outstanding thinkers well versed in social science methodology and familiar with the Western and Eastern professional literature. Since the three books under review address different periods in, and aspects of, recent Polish and East European politics and socioeconomic affairs, they do not lend themselves easily to direct comparison. To their credit, none of them fits comfortably into the conventional comparative politics literature; and all add to our knowledge of "developed socialism" and/or political transitions.

The fundamental thesis of Roman Laba's book is that in contrast with the prevailing consensus in Western (and even East European) academic circles, workers, rather than the intellectual elites, played the central role in fashioning the Solidarity movement. From Leszek Kolakowski to Adam Przeworski, the overwhelming majority of scholars have considered the group of intellectuals who established the Workers Defense Committee (KOR) in 1976 as the critical factor in laying the groundwork for Solidarity in 1980. Not so, maintains Laba, who attempts to demonstrate that the roots of Solidarity should be found in the workers' revolt in the Baltic cities of Gdansk, Gdynia, and Szczecin in December 1970.

The book is divided into two parts: a painstakingly detailed history of the seven days of the 1970 uprising and an "anatomy" of the democratic movement in Poland. As a graduate student, Laba spent two-and-a-half years there in the early 1980s, gathering materials and interviewing the participants of the strikes before he was arrested and expelled from the country for "activities noxious to the interests of the Polish state." His research is admirably thorough and exceptionally focused; and as a result, his analysis is firmly based on unique and original documents. Laba does not question the contribution of intellectuals like Jacek Kuron to the founding of Solidarity (although he neglects to say what this contribution was); but he insists that the definitive role was played by the workers and the autonomous political learning process that they commenced in 1970.

Although he puts forth a strong case, his argument appears to be flawed on several counts. First of all, despite his rather cursory treatment of the social background of workers, Laba seems to have a unidimensional view of the Polish working class—which, by the way, acted much less valiantly in 1968 (when workers attacked the students protesting the communist government's policies) and at other crucial junctures of recent Polish history. For instance, he does not clearly differentiate between workers on the basis of education, income, or party membership. Second, much of his argument rides on the repeated assertion that KOR intellectuals "were almost entirely unaware of the organizational and programmatic achievements of the coastal workers" (p. 94). Yet Laba fails to tell us what these achievements were, aside from the workers' call for free trade unions. One gets the impression that in spite of Laba's spirited analysis, the strikes of 1970 were spontaneous and ill organized. He himself admits, for instance, that many, if not most, members of the strike committees were self-appointed party members and that intellectuals (an engineer and a judge) had advised the strikers (pp. 67, 82); yet he praises the organizational achievements of the workers and contends that intellectuals played no part in the events. Such sweeping statements as that Solidarity was the result of "forty years of conscious but anonymous struggle in Polish factories" (p. 155) not only do not ring true but also fail to enhance his analysis. Although Laba's argument is not entirely convincing, he succeeds in challenging our earlier assumptions about the role of the Polish working class. His treatment of civil–military relations during the crisis offers new insights that contradict our earlier hypotheses and strengthen our understanding of communist armies. This is an exceedingly well written book that no future work on the Polish labor movement can ignore with impunity.

Bartlomiej Kaminski's study analyzes the failures of state socialism and examines the final years of the moribund Polish communist system in that framework. His premise is that "communism, or state socialism, was a terrible idea awfully well implemented" (p. 3)—in contradistinction to the apologists for these regimes, who lauded the soundness of the basic ideas but lamented their poor implementation. Kaminski argues that the roots of collapse were neither political nor economic in nature: instead, they were institutional and systemic. The first half of the book is a theoretical examination of state socialism that attempts to find answers to such questions as, "Why has a system that is so flawed on so many counts managed to survive so long?" (p. 13). The second part is an empirical analysis of Poland in the 1980s.

Prior to becoming a professor of political science, Kaminski taught economics in Warsaw; and his detailed examination of the deficiencies of the state socialist systems in general (and Poland in particular) leave no doubt about his firm and extensive knowledge about both dimensions of his subject. He examines the fundamental shortcomings of the institutional designs of these systems and offers an instructive analysis of how these flaws have affected their political and economic developments. Kaminski lucidly demonstrates the obstacles to the reformability of communist states and the limitations imposed on central planners—and, in turn, their inability to assure economic efficiency. Through the case of Poland, he explains how protracted crisis management had contributed to the decline and, ultimately, the disintegration of state socialism. His writing is at its strongest where he examines political resolutions and contrasts them with the expected and the realized economic consequences. The best point of the book may well be Kaminski's contention that there are *hundreds of different ways* to organize a modern society, a refreshingly broad-minded view considering the rigidity of most East European reformers (p. 211).

Still, much of the book covers territory that is by now quite familiar even to occasional observers of the Eastern Europe. Reading the book, one often asks oneself whether the extensive examination of performance indicators, investment priorities, consumption patterns, and the like from the 1950s, 1960s, and 1970s is really necessary to sustain the otherwise eminently sensible (although rarely original) arguments of this study. Kaminski utilizes a large body of economic statistics and public opinion polls from the 1980s; but the cutoff point of his data is 1987—clearly a pity in view of the profound changes that transpired in the following years. Regrettably also, even though this volume was published in 1991, its author must have completed it before the June 1989 Polish elections, thereby depriving himself and his readers of newly available primary sources and a rich body of interpretative literature that could have informed his analysis.

Jadwiga Staniszkis, a distinguished sociologist at the University of Warsaw, has already acquired a well-earned reputation as an original thinker and social critic on this side of the Atlantic. Her first book published here, *Poland's Self-Limiting Revolution*, offered a perceptive analysis of the birth of the Solidarity movement. Staniszkis' new study confirms and amplifies her scholarly standing; for this is an innovative, insightful, and theoretically complex examination of the collapse of state socialism and the current challenges and future prospects of postcommunist states. Remarkably, these essays were written "on the go" *as* events unfolded; and the author did not substantially rework them prior to publication, thereby bravely giving her readers the opportunity to evaluate her predictions. She comes through with high marks. It would be unfair to Staniszkis to merely list the subheadings of her essays that deal with privatization, the emerging postcommunist social dynamics, the role of society in the revolution, etc., because this book offers much more than what titles suggest.

I cannot here do justice to the awe-inspiring array of arguments, hypotheses, observations, and comparative data featured in this study. In theoretical terms, her discussion is clearly superior to most of the recent and often intellectually conventional volumes on these topics. For instance, she considers the East European breakthrough "as a sequence of three moments: systemic contradictions within socialism, unique historical circumstances and chance phenomena, and the new contradictions within the emerging system" (p. 1). Staniszkis does not view the new East European states as functioning democracies but, rather, as *Standestaat* ("estates' states," a term used to depict late feudal Germany) characterized by the political domination of several distinctive groups (estates) with separate sets of rules but no clearly definable constituencies or economic interests. She also offers new insights into the Kremlin's handling of the East European revolutions (p. 85) and occasionally provides judicious practical suggestions to the current elites, as when she maps out possible privatization strategies (pp. 39–69).

The title of Staniszkis' book is misleading, however. This book certainly does not explain the "breakthrough in *Eastern Europe*" for the reader will find little here that is relevant to Bulgaria or Romania (let alone the other Balkan states). It is a pioneering treatment of the "Polish experience" and greatly advances our understanding of the other East Central European states. The book features a number of essays written, presumably, as intellectual responses to the unfolding events in Poland and in Eastern Europe. While individual chapters (the original essays) are well structured, the volume as a whole lacks cohesion. The most noticeable and irritating problems with Staniszkis' volume are the plethora of typographical errors, inadequate footnoting, and markedly poor translation. These shortcomings, however, are dwarfed by the exceptional merits of this study. Staniszkis moves in and out of political, sociological, and economic theories as she contrasts East European developments with West European ones with impressive ease. Furthermore, as a former adviser to Solidarity and to Lech Walesa, she should be commended for the evenhandedness and objectivity of her work.

University of Texas, Austin ZOLTAN D. BARANY

Economic Rationalism in Canberra: A Nation-building State Changes its Mind. By Michael Pusey. New York: Cambridge University Press, 1992. 310p. $59.50.

This book is designed as both an analysis and critique of economic rationalism among the senior executives of the Australian federal public service and the broader implications of its influence on state and society. The larger of the book's two parts concentrates on the characteristics of the Australian senior executive, based on 215 interviews conducted in the mid-1980s. Specific chapters deal with their images, profiles, and variations by departmental type and with reform in the public service generally. The main argument is that the senior-executive level of the public service has been taken over by economic rationalists. Their background (elitist) and training (a narrow brand of economics) is meant to be reflected in the attitudes they bring to their work.

The examination of senior executives is set within a broader statement about the changing role of the state and the significance of the policies of economic rationalists for Australia. This forms the second part of the book, where these broad issues are expressed more cogently through the author's concern with theorizing about the role of the state and how this has been changing in relation to society. The relative importance of the economic, political, and social orders is argued to have changed, with the economic displacing the social as the object of politics. The relative autonomy of the state has been diminished by internationally oriented economic rationalists exposing the economy to external influences. These "switchmen" are not, of course, confined to the senior civil service but comprise a range of interests concerned with setting new directions for Australia by overturning traditions that accorded prominence to protectionism and social policy and justice.

One can find both merit and problems with the basic line of argument and how it is developed. The book is notable for three accomplishments. First, it attempts a penetrating analysis of the Australian bureaucratic elite, seeks to link this analysis to an examination of broader issues of the state under conditions of major change, and diagnoses a number of issues worthy of serious consideration (e.g., the fate of the career public service). Second, the central argument about the ascendancy of the economic rationalist is well grounded; and the influence of central agency economists in the 1980s in taking the initiative, exercising systemic influence, and redirecting the nation is well established. The role of the economist in the civil service is possibly greater in Australia than in any comparable country; and their contemporary dominance needs to be scrutinised, analysed and debated. Third, the book was published at a time of mounting concern with economic policy failure (producing recession and double-digit unemployment) and the influence of the economic rationalist within the bureaucracy and government generally. The book played a catalytic role in crystallizing public debate nationally, registering more interest in Australia than any comparable publication of recent years.

The book also has a number of shortcomings that detract from its overall significance. Part of the problem lies with the complexities of this ambitious study—multiple themes, an often dense mode of conveying the message, and a tendency to force the data to fit the argument. The argument rests on Pusey's case about the character and influence of a distinctive type of public service elite. This is complicated by the lack of any definition of economic rationalism. Apparently, anyone who has undertaken some economics (or business studies) at university qualifies as an economic rationalist. That those interviewed came from a sample of departments (covering the most economically oriented) inflated the economists' representation among the senior executives interviewed. At times, the author's generalizations extend to the senior executive service (SES) as a whole, although based on interviews with a seventh of them. More problematic is Pusey's attempts to depict the SES as relatively autonomous and conservative in order to sustain his argument about the fate of social democracy in Australia.

An example occurs when he disputes the indications that bureaucrats were being more responsive to ministers: "There is some scope to read the evidence the other way and thus as an indication of the measure in which the economic rationalists have brought their ministers to heel" (p. 75). The evidence to support this argument is not presented. But then, much of the book is contemptuous of the simple idea that the public service has been seeking to address the economic conditions of the 1980s and that pragmatic bureaucrats have been responsive to the government's policy preferences. Also, while the 1980s are identified as a turning point in Australian development, the case is marred by introducing, as a benchmark for comparison, the short period in office of the previous Labor government (1972–75). The reader is offered a rosy, even romantic, version of the public service as it might have operated in the past.

Pusey undermines the credibility of his case by pursuing arguments that cannot readily be sustained. Thus, the bureaucratic elite is claimed to be the product of a distinctive "pattern of differentiation and selection" (p. 53), even though the evidence does not consistently provide support for this. For example, the finding that 74% of senior bureaucrats had fathers with managerial and professional jobs (Aberbach, Rockman, and Putnam's *Bureaucrats and Politicians in Western Democracies* [1981]) is compared with the Australian figure of 27% and described as being "not in quite the same measure as their counterparts overseas" (p. 50). Further examples could be cited of the data being forced to fit the depiction of the conservative character of the SES.

This is a complex study operating at several levels. How one responds to the book may well depend on one's stance. For the social democrat, the passion and the concern with national social issues is attractive. For the sociologist, the use of theory to illuminate change may be well received. The political scientist will be both stimulated and disappointed by the analysis.

University of Canberra JOHN HALLIGAN

Social Mobility and Political Attitudes: Comparative Perspectives. Edited by Frederick C. Turner. New Brunswick: Transaction, 1992. 297p. 39.95.

This is a series of excellent studies about a very important subject, the effect of social mobility on political attitudes in ten contemporary nations—not including the six East European ones claimed by the dust jacket. It presents relevant survey research of the years 1955–84, elaborated by members of the IPSA Research Committee on Comparative Public Opinion and commented on by

S. M. Lipset and the editor. These were years of great change in the experience and subjective political interpretation of social mobility in all the nations covered, whether industrialized or Third World. This fine book only begins to mention some of the dimensions and questions raised by this complex subject, such as the crucial role of subjective perceptions and experiences of mobility, and the role of the profound conflicts that have made this such an explosive topic.

Some of the contributors confront the soaring sociological imaginations of Karl Marx, Alexis de Tocqueville, Ralf Dahrendorf, Lipset, Reinhard Bendix, and others with the sobering need for conceptual clarity and operationalizability for empirical research (pp. 9–10, 22). In their dedication to operationalization, however, they fail to account for the likelihood that the subjective, polarized reality of "class struggle" in Marx's day, in the 1920s or 1930s, or even in the 1950s may have been very different from the placid, post-Marxist 1980s. There is a lot of information here about the objective changes in class "categories" and status levels but very little about the life-and-death struggle of rising European labor movements against feudal and bourgeois resistance or of the countercoups attempted under the generic names of fascism and reaction against these class-conscious upstarts. Instead, we are given a methodology-rich but ahistorical analysis by Jonathan Kelley of measurable effects of individual upward and downward mobility on recent voting choices between the moderate (not the extreme) Right and Left. Somehow, the original emphases of a Marx, Lipset, or Werner Sombart disappear amid the elusive notions of 1980s-style "conservatism" and the welfare state, not to mention some glaring gaps in comparable data from nation to nation and the failure to stress the different historical experiences of, say, Britain and the United States in an earlier day. The much-repeated upshot of these studies is that intergenerational social mobility per se appears to have no noteworthy impact on political attitudes, although the class origins and class destination of the mobile do.

There is a kind of eclipse of the political nature of "class" as a focus of group identification, a collective battering ram against established hierarchies of authority, and a flag behind which to rally the mobilized masses. Class has no life outside the minds of the principals; and its subjective, political interpretations differ widely among different nations and during different historical eras. Marx himself, of course, had depoliticized social forces by insisting that it was the objective conditions that drove revolutionaries into the great confrontation, rather than that the latter might fashion a movement from their interpretation of the conditions. Fortunately, some of the essays, especially those on Germany, Denmark, Norway, and Japan revive the post-Marxist political significance of the mobility issues by resolutely placing them into the contemporary context—the contradiction between the "inevitability of stratification" and the egalitarianism professed by democracy, a contradiction that may weaken the legitimacy of the system (pp. 51, 158). The dramatic decline of the class-conscious parts of the working class as compared to non-class-conscious workers (e.g., in Norway, 1965–85; see p. 115) also reveals the passing of the Marxist discourse from the political stage while social equality remains an issue. The editor deserves high praise for bringing these select papers before an appreciative audience.

The book also includes essays on mobility and politics in India, Nigeria, and Argentina that remind us of the extraordinary richness of this topic in settings different from the industrialized democracies discussed earlier. The drastic pace of change and the resistance of ascriptive elites to the upward advance of whole classes (as distinct from Kelley's individual mobiles) still has its parallels in nineteenth-century Europe. But the complex interaction of social mobility with multiethnic rivalries and the peculiar mobility dynamics of immigrant societies (a subject curiously absent from the account of the United States and Australia) create rather different conflicts from what the Marxist discourse suggests, another reason why interpretations in terms of class conflict are not always adequate to certain societies.

When Marx turned Hegel's "idealistic" interpretation on its head, he thereby created new contradictions between the alleged social reality and its reflection in the minds. From that moment on, at least on issues of class, the flickering lights and shadows on the walls of Plato's cave have become more puzzling than ever. Microscopic examination of these lights and shadows, however, will not bring us more insight without historical reflection.

University of California, Santa Barbara PETER MERKL

INTERNATIONAL RELATIONS

Free Movement: Ethical Issues in the Transnational Migration of People and of Money. Edited by Brian Barry and Robert E. Goodin. University Park: Pennsylvania State University Press, 1992. 300p. $45.00 cloth, $16.95 paper.

This book considers two transnational movements in international justice: that of people and that of income and wealth. Often considered separately, questions about immigration policy and about a state's international distributive rights and responsibilities are, in fact, closely related at both the theoretical and the policy levels. One of the main virtues of this book is to recognize these relationships and make them a subject of discussion.

All but two of the papers in this volume were originally prepared for a conference held in September 1989 at Mont Saint Michel and organized by the Ethikon Institute, a small, California-based organization devoted to increasing tolerance and understanding of different traditions of moral thought. The distinguishing feature of the institute's approach is to explore how a common set of problems (in this case, those arising from international movements of people and money) appears from a variety of theoretical perspectives: liberal egalitarianism, libertarianism, Marxism, natural law, and political realism. If there is an overarching philosophical aim, in addition to advancing our grasp of the two sets of substantive problems both on their own and in relation to each other, it is to see whether and how the type of normative theory that one accepts influences one's views on matters of policy—a question of obvious philosophical, as well as practical, interest.

The book consists of five main sections, one for each of the theoretical perspectives. Each section contains one paper on migration of people and one on transfers of money and a commentary. The intent was that the lead papers in each section should explore the application of that section's theoretical perspective to the issue at hand, while the commentary would both criticize and probe connections between the perspective's approach to the two substantive issues.

Although this structure suggests a grand, even rigid, symmetry, the volume is, in fact, somewhat uneven. International migration is treated in considerably more philosophical depth than international capital flows. Among the five theoretical perspectives, only three (liberalism, libertarianism, and natural law) are really carried through as the overall program would seem to require. Accordingly, the overarching aim of testing the connection between theoretical commitments and views about policy is only partly realized.

There is, nevertheless, much of value in this book. Indeed, the sum of the parts may be greater than the whole. For example, there is an interesting exchange between Joseph Carens and James Woodward as to whether (as Carens argues) a liberal egalitarian view of distributive justice argues for significantly less restrictive immigration policies than those typically found in the industrial democracies. Hillel Steiner's paper on libertarianism and immigration policy advances a provocative view about the policy question and also contributes valuable observations about the interpretation (and misinterpretation) of libertarianism. There is a good paper by Paul Weithman on natural law and international justice and a fine commentary (really an original paper) by Philippe van Parijs best characterized as an attempt to address both sets of normative issues from a perspective inspired by (but only problematically within) the modern Marxist tradition.

With the exception of the paper by Weithman, philosophically inclined readers are likely to find more of interest in the papers on immigration policy than in those on international capital and income flows. Some of the latter (e.g., those by I. M. D. Little and Deepak Lal) are engaging in their own right; but the major normative questions are less successfully joined than in the papers on migration of people. The reason, I suspect, is that these questions are less well defined politically and that the policy instruments to alter prevailing international flows of income and wealth are both complex and obscure. There has been considerable progress in the last decade in the development of views about principles of international distributive justice; but as these papers illustrate, the difficult problems of application (really problems for normative international political economy) still await their proper formulation.

The volume as a whole was stimulated by the perception that governments seem to take contrasting views of the two kinds of movement considered here: they restrict inflows of people but welcome inflows (and resist outflows) of money. There is room to wonder why one would regard this as noteworthy. Perhaps, as Robert Goodin suggests, it was thought that governments should address these issues in some way consistently. But this thought itself is puzzling: just as the immigration of people and the immigration (or emigration) of money pose different political issues and generate different coalitions, so they pose different theoretical problems (not *entirely* different, of course, but different enough to matter). For example, flows of people have the potential to bring about rapid and destabilizing changes in the character of political and social life and in the distribution of income and wealth within the state. The domestic distributional and social effects of transnational flows of money, on the other hand, are second-order and hence more easily detached from the flows themselves. Thus, concerns that might be classified as broadly communitarian are only indirectly implicated in thinking about international distributive justice but could be decisive in discourse about immigration policy. While, in the end, the impression may prove to be wrong, it should not seem unlikely at the outset that a philosophically consistent view about these subjects could generate asymmetric policy conclusions.

In spite of its unevenness (not surprising in a conference volume), this book contributes valuably to the literature about international justice, particularly in connection with immigration questions. Interested readers will find it provocative and helpful in their capacities as theorists and teachers.

Bowdoin College CHARLES R. BEITZ

Soviet Strategic Arms Policy before SALT. By Christoph Bluth. New York: Cambridge University Press, 1992. 317p. $59.95.

Black Earth, Red Star: A History of Soviet Security Policy, 1917–1991. By R. Craig Nation. Ithaca: Cornell University Press, 1992. 341p. $29.95.

For much of the period between the late 1940s, when a distinctly *Soviet* variant of comparative politics first emerged within American political science, and the present, experienced observers of the Soviet political scene have frequently found themselves at odds with their counterparts in the field of international relations over how best to account for the sources of Moscow's international conduct.

The comparativists, led, throughout much of the Cold War, by such respected analysts as Herbert Dinerstein, Adam Ulam, Carl Linden, and Michel Tatu, consistently located the sources of Soviet conduct in the complex and ever-changing interplay of domestic political, social, and economic variables. To the extent that external, or international-level, variables were seen to be important, the place assigned to them was almost always secondary to, or derivative of, the first group of factors. International relations theorists, to the degree that they troubled themselves with the issue at all, just as consistently sought to cast light on the Soviet Union's behavior in the international system by reference to the logic of the system itself, that is, to treat the Soviet Union as a conventional (albeit a very large and rather unusual) state actor, whose leaders, from Lenin to Gorbachev, made policy in response to the same set of factors—the same combination of constraints and opportunities—that historically have guided other states in the conduct of their external relations. A spate of more recent scholarship on the Soviet Union, including that of George Breslauer, David Holloway, Robert Legvold, and Jack Snyder, has attempted to meld the two perspectives in novel ways, bringing together the "rigor" associated with at least some international relations theory and the "richness" more often characteristic of high-quality Sovietology.

This long-standing and inconclusive debate is about to be enlivened by the appearance of two new studies, by Christoph Bluth and R. Craig Nation, on the development of Soviet security policy. Both books, to different degrees, come down on the side of domestic-level explanations. On balance, they do so convincingly and in such a way as to strengthen the hand of those who argue that what Soviet-style comparative politics really needs is greater richness, which, in time, one presumes, will also generate greater rigor.

The more ambitious, if less controversial, of the two books is Nation's *Black Earth, Red Star.* Nation sets out to provide a self-contained, internally consistent, and fair-minded account of the history of Soviet security policy. For the most part, he succeeds. In three hundred pages, Nation traces the development of Soviet policy on security-related issues from the first, chaotic moments of the Bolshevik regime in 1917 to the final, agonizing days of the once mighty Soviet state in 1991. Along the way he recounts, with admirable clarity and genuine insight, the enduring tension felt by successive Soviet leaderships as they sought to balance the commitment to internationalize, and thus validate, their own revolution with the need, first and foremost, to provide for the physical security of the Soviet state (which, for most of its troubled history, confronted an alliance of extremely powerful and uniformly hostile capitalist countries committed to its eventual elimination).

While Nation connects much of what actually *happened* in Soviet security policy to the leadership struggles that so dominated Soviet political life, in particular between 1917 and the ouster of Nikita Khrushchev in 1964, he is, at the same time, careful to explain how decisions reached in Moscow were heavily conditioned by the external environment, including such factors as the military power of the Soviet Union relative to its imperialist rivals, the perceived strength or weakness of the international capitalist economy, and the likelihood of a major war.

The book, which rests on a broad base of empirical sources, both primary and secondary, as well as works published in four languages, is thematic and synthetic in character; as such, it breaks relatively little new ground. In revisiting terrain that is familiar to most students of the subject, however, it reminds us how much of what we think we know about Soviet security policy is episodic—tied to specific events and particular actors—and how important it is, especially now, to begin to reconceptualize our understanding of, and deepen our knowledge about, the Soviet experience. If *Black Earth, Red Star* suffers from any weakness, it is the attempt to tell too much in too few pages (the implicit decision to sacrifice depth for breadth) and the extremely brief treatment it accords Mikhail Gorbachev's attempts between 1985 and 1991 to engineer nothing less than a total revolution in Soviet foreign and military policies. These problems notwithstanding, *Black Earth, Red Star* is a work of mature scholarship and the most important book on the history of Soviet security policy to appear in print since the first edition of Adam Ulam's monumental *Expansion and Coexistence* (1967).

Christoph Bluth's *Soviet Strategic Arms Policy before SALT,* which concentrates on the development of Soviet military strategy and strategic weapons programs from 1953 to 1966 is a very different kind of study. If Nation first surrounds, and then lingers over, his subject, Bluth focuses on his with the intensity of a laser. Moreover, Bluth firmly alleges, that the key to understanding Soviet security policy outcomes during this period is the constant struggle for preeminence between the country's political leadership and a "military industrial complex" comprised of the armed forces, defense industrialists, and their allies in the Communist party bureaucracy. Nation, while not unsympathetic to this explanation, ultimately opts for a more circumspect (and satisfying) conclusion in which domestic- and international-level variables constantly combine and recombine to generate particular policy outcomes.

In recounting the story of the Soviet Union's rise as a military superpower through the prism of domestic politics, Bluth self-consciously harkens back to, and relies for inspiration on, among other works, Carl Linden's *Khrushchev and the Soviet Leadership, 1957–1964* (1962) and Michel Tatu's *Power in the Kremlin: From Khrushchev to Kosygin* (1967), both of which look to the personal and political machinations of senior leaders in Moscow to explain the content and conduct of Soviet foreign and military policies. While there is much to be said in defense of this domestically grounded approach, Bluth is too quick to dismiss the utility of other kinds of explanations for Moscow's behavior during these years, such as the "action–reaction" model of U.S.–Soviet strategic interaction, which, while less than completely compelling in its own right, has more merit than Bluth suggests. As is so often the case in works of this kind, the effect of Bluth's excessive stridency in support of both his argument and approach is to weaken the very case that he is trying to make.

A second shortcoming is Bluth's tendency to remain too deeply mired in the minutiae of the story he wants to tell. This is less of a problem for specialists than it is for readers with more general interests; but given the manifest need for all who write on these and related issues to situate their findings within the larger literature on the sources of the Soviet Union's international conduct, the failing is a real one. The book is generally well written but at least a third again as long as it needs to be. A different kind of organizational scheme, in which Bluth would not be required to tell parts of the same story over and over again, would have significantly improved the final product. Though limited in scope and occasionally repetitious, *Soviet Strategic Arms Policy before SALT* is a useful addition to the growing body of work on the postwar conduct of the USSR in this important area; and until Western access to Soviet archives improves, it is likely to remain one of the better and more informed analyses available.

Alas, neither of these books will resolve the debate between comparativists and international relations theorists over how to understand the formation and conduct of the Soviet Union's external policies. Neither, of course, had this as its central purpose. The need for such a work remains, nonetheless; and I, at least, eagerly await its publication.

Stanford University COIT D. BLACKER

War and Reason: Domestic and International Imperatives. By Bruce Bueno de Mesquita and David Lalman. New Haven: Yale University Press, 1992. 322p. $40.00.

War and Reason is an important book that merits special attention and close scrutiny from the international relations research community. Using recent advances in non-cooperative game theory, Bueno de Mesquita and Lalman explore the theoretical implications of an "international interactions game" that is unusually suggestive of the "stuff" of international politics. The model's richness stems from an intuitive game structure and a broad range of distinguishable outcomes. As one might expect, particular attention is focused on two of the game's proper subgames. These (crisis) subgames occur whenever one state resists another's demand for an alteration of the status quo. Five of the larger game's outcomes are crisis dependent: capitulation (by either player) after a show of force, waging war on an opponent's terms, or bilateral negotiation. The remaining two outcomes are unique to the larger game: acquiescence by either player, or the status quo. As the authors assert, this game model "captures a great deal of international relations and extends well beyond the field of conflict studies" (p. 31).

A small set of plausible assumptions guides the analysis. Players (states) are assumed, *inter alia,* to be expected utility maximizers; they know the consequences of capitulation with certainty, but negotiation and war are seen as risky events with uncertain outcomes; states prefer acquiescence to capitulation.

More problematic is the assumption that the domestic cost of using force rises monotonically with the probability of success in a conflict. Were this postulate universally satisfied, one would anticipate very high political costs associated with relatively small events like the Mayaguez affair or the U.S. invasion of Panama in 1989, and far lower costs in more treacherous arenas. Also troublesome is the presumption that negotiations are always preferred to war. This assumption eliminates situations like the one that existed prior to the Japanese attack on Pearl Harbor. In 1941 the expectation that negotiation with the United States would be futile, or worse, led the Japanese to choose war.

But these are quibbles. Assumptions are best judged by their ability to generate interesting and empirically supported propositions. On this score, *War and Reason* excels.

The most interesting conclusions regard two competing sets of propositions deduced from the model. Little empirical support is found for what the authors term the *realist* variant in which no constraints are placed on the demands and counter-demands available to the players. Bueno de Mesquita and Lalman show that without such constraints the only rational (i.e., subgame perfect equilibrium) outcomes are the status quo and negotiation. Clearly, to explain the broad array of observed events, additional assumptions are necessary. The required restrictions on preferences are attributed to the vagaries of domestic politics. Significantly, strong empirical evidence is adduced to lend credence to the authors' claim that the *domestic* variant of the model is largely consistent with the historical record.

This is an important result. It proves logically, and demonstrates empirically, what many researchers have suspected all along, that is, that structural realism is largely indeterminate. Without granting license to disregard systemic factors or power relationships—as many of the case studies make clear—it highlights the critical role played in international politics by factors that are either ignored by neorealists or relegated to the infamous black box. More important, by specifying the conditions necessary or sufficient to induce various outcomes, it helps to focus attention on the particular internal debates and domestic processes critical for understanding interstate interactions.

Almost as interesting is the discussion of the role that norms and beliefs play in averting international conflict. The authors are too quick, however, to write off the possibility of cooperation in prisoners' dilemma games. Contrary to Snyder and Diesing's finding in *Conflict Among Nations* (1977) that crisis games that share the structure of prisoners' dilemma normally end in compromise, they find that war "is more common when the conditions conducive to tit for tat are met than when they are not" (p. 142). Since the model predicts war under the conditions that are associated with this game, it is taken as additional empirical support for the underlying theory.

The problem lies not with the supporting evidence but with the definition of the game. As is well known, prisoners' dilemma is a symmetric game. Yet the preferences the authors associate with it are decidedly asymmetric. Specifically, in identifying a prisoners' dilemma situation within the larger game structure, Bueno de Mesquita and Lalman assume that the challenger prefers capitulation to fighting a war the defender initiates, while the defender has precisely the opposite preferences. It is easy to demonstrate that the (war) equilibrium of the crisis subgame is extremely sensitive to this asymmetry. (If the challenger's preferences are reversed and brought in line with the defender's, negotiations are implied; and if the defender's preferences are altered, it should capitulate. In neither instance does war remain rational.) Thus, while the empirical findings should still be interpreted as corroborating evidence for the theoretical structure the authors advance, their conclusion about the unlikely possibility of cooperation in prisoners' dilemma games is open to debate. In fact, just the opposite conclusion can be drawn from the discussion of the "self-defense" norm which, like the standard rendition of prisoners' dilemma, is defined in terms of each player's preference for conflict (i.e., "punishment") over capitulation (i.e., the "sucker's" payoff). As the authors show, when information is complete, "if both parties to a conflict of interest are known to be prepared to retaliate if attacked, then cooperation or harmony is guaranteed" (p. 124).

The theory's organizing power is demonstrated in an enlightening explanation of why democracies, as war-prone as any other type of state, rarely fight one another. Perhaps even more instructive is the empirical comparison of the game model with balance of power and power preponderance theories. (The hegemonic theories fare rather well; balance of power does not.) At times, however, the interpretation of aspects of these theories is puzzling. For example, Bueno de Mesquita and Lalman claim that preponderance theorists assert that system transforming wars must be large wars. A chapter length discussion of the Seven Weeks War, which is fascinating in its own right, is even devoted to illustrating the proposition that small wars may have major consequences. But the underlying claim seems strained. After all, power transition theory (Organski and Kugler, *The War Ledger*, 1980) admits the possibility of peaceful transitions. Why then should the authors be surprised that a transition war, especially an intra-coalition war, be limited? This objection aside, the game model's ability to more fully account for the outbreak of war than either power transition or balance of power mark it as a major theoretical advance.

What place will *War and Reason* hold in the scientific literature of interstate relations? Using Lakatos' criteria, the game model is shown to be superior to the decision-theoretic framework articulated in Bueno de Mesquita's *The War Trap* (1981). It is clearly the better book. Nevertheless, it is unlikely to have the same dramatic impact, simply because *the War Trap* came first. *War and Reason* will probably be thought of as a significant addition to a rapidly growing literature that employs axiomatic theory to understand international conflict and its resolution. It may even come to be seen as *primus inter pares*. But its influence will likely be modulated because the rational choice paradigm, now more than ever, constitutes a progressive research program with many of the characteristics of a maturing science. Under such conditions, revolutionary contributions are most improbable.

Still, *War and Reason* is an innovative and far-ranging work that redefines the state of the art and extends our knowledge about interstate conflict and cooperation. It is the most significant application to date of game-theory to the question of war and peace. In a very real sense, it actualizes the considerable potential of this methodology, not only to highlight fundamental questions about international politics, but to probe deeply into their solution.

State University of New York, Buffalo FRANK C. ZAGARE

Rival Capitalists: International Competitiveness in the United States, Japan, and Western Europe. By Jeffrey A. Hart. Ithaca: Cornell University Press, 1992. 305p. $32.95 cloth, $16.95 paper.

Rival States, Rival Firms: Competition for World Market Shares. By John M. Stopford and Susan Strange, with John S. Henley. New York: Cambridge University Press, 1991. 321p. $59.95 cloth, $18.95 paper.

In these two new books, Jeffrey Hart, John Stopford, and Susan Strange carry the argument about "competitiveness" further—though the Stopford–Strange volume is as concerned with the competitiveness of firms as it is of nations. The two works draw their evidence from varying national contexts: Japan, the United States, Britain, France, and Germany (Hart) and Kenya, Brazil, and Malaysia (Stopford and Strange). Given their different centers of attention, the two books reach proportionately different conclusions. From a developing-country perspective, Stopford and Strange see states as weak, multinationals as strong. The question for them is how a relatively poor country can induce reluctant multinationals to invest within its territorial confines, bringing in new capital and technology. Hart's argument is more traditional: How do strong countries and governments improve the productive efficiency of their societies as a whole? Both, however, have to confront the problem that (with certain key exceptions) the bargaining power of governments as heads of national society has declined in comparison to that of large international corporations.

Hart modifies the preexisting wisdom by contending that the particular content and direction of state–society relationships determine the outcome in international competitiveness. Generally speaking, German success stems from the labor–business partnership; the Japanese miracle is due to the government–business link. Less successful are the pure (unalloyed) types, like the preponderant labor orientation in Britain, state dominance in France, and business precedence in the United States. If this conclusion is correct, the United States should modify its free-market orientation and move toward a mixed relationship of business and government, while France should dilute its statist policies in a new partnership with industry. Britain also needs to shift either toward government or business to improve its competitive performance.

Both books recognize that technology, capital, and a trained labor force are critical to winning the race, at either firm or government levels. Both understand that one has to negotiate with other states, as well as foreign firms, if success is to be achieved. But neither book quite tells one how to do it. For example, should a government seek to create a free-trade area, favoring firms that operate within the bloc? Should it sanction *keiretsu* arrangements for its own firms, so that companies within an industrial group can gain preferred access to capital and technology, strengthening them against foreign competition? How should a government use its administrative and regulatory powers to protect the position of its own industries? In influencing foreign treatment of its exports and multinational corporations, should a government engage in specific, or only in diffuse, reciprocity with foreign nations?

Stopford and Strange rightly conclude that unfettered markets do not necessarily produce the results one wants, as witness the comparative decline of both Britain and the United States, the most liberal and open of modern economies. Hart agrees and observes that "corporatism," "culture," "statism," and "coalitional" explanations also fail the test, because, as independent variables, they do not vary with change in the dependent variable (competitive success). But Hart's own state-society explanation is vulnerable on the same grounds. Underneath the general social formulas, there must be other specific causes that are being neglected in both analyses.

In fact, the world economy today is still largely politically dominated and controlled, not by a hegemonic power but by an oligopoly of states. Each member needs to maintain its market share, but none wants to risk a game of ruin with others—depressing the world price and threatening its own productive future. Thus, each is forced to respond to others when trade or financial flows become too unbalanced. In this sense, losers are occasionally compensated by winners and helped to regain their footing. Russia is, or soon will be, the beneficiary of others' indulgence. And before long, the American turn will come; for the United States boasts the largest trade imbalance and the highest degree of indebtedness of any country. The United States will demand, and probably receive, special treatment, contingent upon its own domestic economic reform. As Thomas Schelling says, in bargaining, weakness is strength. Both books unfortunately neglect such bargaining perspectives, though such strategies (hegemonic, free-rider, or reciprocator) appear to vary with the outcomes in national prosperity and competitiveness that they both depict.

This, of course, does not entirely solve the problem, because governments (with the possible exception of the government of Japan) cannot exert a decisive influence upon their own firms. And even in Japan, *keiretsu* independence seems to be growing. Why is this? And is there anything that can be done about it? The present weakness of governmental policy is probably the effect of a technology that puts more and more emphasis upon factors of production that governments cannot fully control. The most firmly fixed factors of production (which are amenable to national fiat) are no longer the most economically important. *Land*, for example, is no longer a decisive ingredient in the most productive value-added technology; and the possession of more land is no great advantage. Thus, governments' monopoly of territory serves them ill. Governments can influence, to a degree, *specific capital*—the industrial assets that are placed within national confines. But these, as Stopford and Strange point out, can be shifted elsewhere if financial incentives so dictate. Even more significant *mobile capital* can shift like quicksilver from country to country. When Saddam Hussein occupied Kuwait City, he found the cupboard bare and the capital flown. *Labor*, of course, is likely to be more stationary and therefore more amenable to governmental control. But in recent years, it has become more mobile, though still not as fluid or fungible as capital. Still, after World War II, the Soviets commanded their labor force with rigorous political controls. Vietnam could not do so, however; and the most economically relevant population, the ethnic Chinese–Vietnamese, went elsewhere after the war ended. In fact, though Japan has not yet fully understood the phenomenon, a country that can induce the most highly trained and industrious labor force to visit its shores has a mighty advantage in world economic politics. Countries today, however, face brain drains, as well as inflows.

The insight that unites the two books is that states cannot guarantee their own economic future. They must negotiate to ensure themselves economically and deal with foreign corporations, as well as foreign governments. As the important factors of production that one wishes to acquire become more *mobile*, this task becomes more difficult. And in the twenty-first century, it will become more complicated still, as *information* becomes the needed desideratum. Service industries that provide information are the most mobile of all, yet they are probably the most critical to future success. That is why the issue is still up in the air and why neither book can hazard a guess on the evolution of competitive development. States have become much more like firms, and the contours of the international political market have yet to be drawn. A good guide to success, however, is to develop the most highly trained and industrious labor force, ease the entry of foreign capital, and assist the development of new technology through government-assisted research and development. The states that succeed will do so because of the success of their firms in international competition. Rival states depend in part on allied firms.

University of California, Los Angeles RICHARD ROSECRANCE

Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965. By Yuen Foong Khong. Princeton: Princeton University Press, 1992. 286p. $39.50 cloth, $17.95 paper.

Diffidence and Ambition: The Intellectual Sources of U.S. Foreign Policy. By Carlo Maria Santoro. Boulder: Westview, 1992. 316p. $57.50.

These two otherwise dissimilar books are part of a growing literature exploring the role of "ideas" in shaping government policy. Santoro posits that national security and interdependence—the principles that guided and legitimized U.S. Cold War policy—were developed in 1940–41 before the Japanese attack on Pearl Harbor, when the United States was pursuing a policy of neutrality. The purpose of his book is to trace the evolution of conceptual precursors through the Council on Foreign Relations' (CFR's) War and Peace Studies Project, which was begun in 1939 as a service to the State Department. Khong contends that the Johnson administration's perception that the Vietnam conflict was analogous to the Korean War led them to intervene in Vietnam in 1964–65 and to do so with less force than officials believed was necessary to accomplish American objectives. His vivid memory of MacArthur's disastrous march to the Yalu caused President Lyndon B. Johnson to reject such options as bombing petroleum and lubricants in North Vietnam or mining Haiphong Harbor. In short, Khong maintains that the inferences they drew from the Korean analogy explain not only *why* the Johnson administration escalated the Vietnam War but *how*.

A central problem with previous studies in this genre is that they fail to uncover the mechanisms by which ideas are translated into action. It is one thing to show that particular theories or concepts were part of the intellectual climate, quite another to demonstrate that policymakers were guided by them in choosing a particular policy. We cannot take at face value the reasons that policymakers give for their actions, because not only may such statements be self-serving, they may be based on conjecture, rather than awareness. There is also the question of how important ideas are relative to the structural conditions that constrain the choices of leaders.

Santoro alternates between systemic and conceptual levels of analysis to show how the changes in the international system brought about by Nazi Germany's victories affected how internationalists defined America's role in the world. If the United States had not been forced to develop an integrated economic and military system to defeat Germany, the American people would have been much more resistant to a permanent U.S. presence in Europe.

For example, after the fall of France, the CFR study group viewed Great Britain as an essential base for overseas military operations when the United States entered the war. The image of Britain as an "advance base" stimulated the development of the axiom that the United States should obtain forward bases from which to attack the vital centers of the enemy while preventing the enemy from doing so. In 1942, the CFR security and armaments group envisioned that the United States would maintain permanent bases around the world, in defeated Axis territory, as well as in Allied and liberated countries. National security was part of a geopolitical and strategic approach that implied that the United States should maintain a permanent state of military readiness, a network of overseas bases, a favorable balance of power, and the ability to intervene with force in any part of the world.

Santoro maintains that the Truman administration, confronted with the failure of Roosevelt's grand design and the need to throw together alternative plans for postwar reconstruction and order, drew on all the previous planning studies drafted during the war. The ideas were then remolded to suit new circumstances; the same concepts used to justify war against Germany were employed against the Soviet Union.

Their haphazard origins exemplified how globalistic concepts were applied in American postwar foreign policy. Because the expansive conception of U.S. security interests derived not from any unified system of ideas but from a collection of contradictory notions, the containment policy demonstrated internal incoherence, veering from legalism to economism to imperialism.

Although Santoro persuasively draws a link between the internationalists' conceptions of the U.S. postwar role and macro-level changes in the international system, he does not make a convincing case that the CFR's studies influenced the Truman administration's decisions. Although Santoro claims to have found textual similarities between records of the CFR and State Department planning papers, he presents only a few examples. Also lacking is any demonstration that these ideas affected the thinking of President Harry Truman.

Whereas Santoro presents a traditional intellectual history locating ideas within their context, Khong provides a methodologically exemplary study of the nexus between analogies and individual decision making. This micro perspective enables him to make a more persuasive case for the causal impact of ideas. The price is the loss of explanatory breadth, because his restricted focus does not encompass the background conditions that made some form of U.S. involvement in Vietnam highly probable.

Khong analyzes in fine-grained detail how analogies influenced the Johnson administration's selection and evaluation of options for Vietnam. Using Alexander George's "congruence method," Khong shows that only the Korean analogy was consistent with the decisions for a graduated bombing campaign and limited ground war.

Khong thus goes beyond the important work of Ernest May, Richard Neustadt, and Robert Jervis on how policymakers misuse the "lessons of history" by showing precisely how analogies enter into the decision-making process and by providing a theoretical explanation. The same analogical reasoning that produces error enables people to comprehend and process information about the world. According to schema theory in social psychology, we match new situations to knowledge structures stored in memory. When no cognitive structure fits the data, we may invoke close matches. People are particularly likely to invoke historical analogies that bear some superficial similarity to the current case. For example, Korea was a natural analogy to Vietnam because it was also a divided country on China's border. The pervasiveness of analogical reasoning in everyday life suggests that we need to rethink prescriptions that foreign policymakers learn more about history or invoke many analogies.

Khong confronts the charge that analogies are used as post hoc justification, rather than analysis of emerging foreign policy situations, by showing that Johnson administration officials repeatedly used analogies in private, even when others challenged their relevance. The view of analogies as mere rationalizations is too restrictive, for it fails to examine their psychological functions.

Khong stops just short of claiming too much for his analogical explanation of U.S. intervention in Vietnam. American involvement in Vietnam was determined by many causal factors, including the bipolar distribution of power, concern for the credibility of extended deterrence, memories of the anticommunist hysteria that followed the loss of China, and beliefs in the domino theory. Because U.S. involvement unfolded over time in response to changing circumstances, focusing solely on the admittedly important decisions of February and July 1965 overlooks these necessary international and domestic conditions.

In the future, we may hope that scholars studying the interaction between ideas and policy will follow Khong's example in uncovering the impact of ideas on the decision-making process while relating individual decisions to the constraints imposed by the environment.

University of California, Los Angeles DEBORAH WELCH LARSON

Arms and the State: Patterns of Military Production and Trade. By Keith Krause. New York: Cambridge University Press, 1992. 299p. $49.95.

The International Arms Trade. By Edward J. Laurance. New York: Lexington Books, 1992. 245p. $35.00.

The year 1992 saw the publication of these two broad-gauged, but also conceptual, works on the arms trade, which together improve the state of the literature on that subject, after a lengthy hiatus that had seen numerous works with a narrower focus. Neither Krause nor Laurance claim to provide a "theory" of or for the arms trade. But both seek to provide virtually all-inclusive frameworks for analysis in emphasizing a longer-range historical perspective—in Krause's case extending back to the fifteenth century, in Laurance's, back to the interwar period. Both rely on variants of systems analysis, that is, on layering of phases of arms trade history.

Keith Krause's book is strong across the board: it develops new historical materials spanning centuries; it attends to the complex interrelationships among international politics, dominant processes of international economics, and technological developments (with the author's obviously strong command of the huge literature on the arms trade and its contextual peripheries); and it analyzes contemporary arms-trade data. Following the recent (salubrious) practice established by SIPRI publications, the author simultaneously deals with arms production and trade data, a healthy corrective to the earlier practice of analyzing the latter somewhat in isolation. Krause works with phases or cycles of the arms trade itself, rather than treating the arms trade as a function of profound changes in the international system or of some key variables characterizing such systems (structural polarity, ideological basis for alignments, etc.).

Krause's analysis is organized around three waves of the "arms transfer and production system." The first, an "early modern period," is said to have been triggered by the introduction of the revolutionary military technologies of gunpowder, cannon and firearms in the early fifteenth century and to have played itself out through the imperfect and slow process of technological diffusion in the sixteenth and seventeenth centuries, when neomercantilist European governments exercised considerable control over weapons exports. The second wave, associated with the industrial revolution, had its genesis in the 1840s, peaked between 1858 and 1888, and reached maturity in the early twentieth century. During this time the whole nature of armaments underwent its greatest period of revolutionary innovation since the development of gunpowder and cannon, featuring steam-powered and steel-clad ships, steel cannon, breech-loaded guns, and the like. It was also a period of laissez-faire arms selling, which produced seemingly odd patterns of the arms trade at variance with prevailing political alignments. The third wave, a post-1945 international arms transfer system, was based on the decisive transformation of military technology emerging from World War II (radar, jet aircraft, missiles) and on now institutionalized research and development, much higher unit costs for weapons, and the bipolar dominance of arms production.

All three of these waves are seen having resulted in rapid diffusion of new technologies and a stratification of weapons production and trade capabilities into three tiers: (1) first-tier countries innovating at the technological frontier; (2) second-tier countries producing, via the transfer of capacities, weapons at the technological frontier and adapting them to specific market needs; and (3) third-tier countries copying and reproducing existing technologies but not capturing the underlying process of innovation or adaptation. This breakdown by tiers is then cross-referenced, for each of the historical waves, to a three-way breakdown of motives for arms sales: wealth, pursuit of victory in war, and pursuit of power. Wealth includes foreign exchange, economies of scale, employment, recouping of research-and-development costs, and "engine for economic development"; pursuit of victory in war includes bases, assistance to allies, substitutes for direct military involvement, testing of new weapons, and autarky to ensure military security; and pursuit of power includes access to foreign leaders, symbolic commitments to allies, regional balances of power, presence, and access to resources.

Krause engages in an extensive analysis, for each life cycle of the arms trade, of the three aforementioned tiers of suppliers. Each cycle is seen initially catalyzed by revolutionary technological innovation and the emergence of centers of advanced arms production, followed by an imperfect diffusion of new technologies giving rise to second- and third-tier producers. This diffusion is not smooth or linear, and the system remains highly stratified even as there is also a tendency toward slowing of innovation in first-tier states in the later stages of each cycle. These waves are all, according to Krause, driven by technological change, not by such major political upheavals as hegemonic wars.

If Krause's work has any weakness, if only by omission, it might be in the relatively short shrift given to patterns of arms acquisitions (who gets arms from whom and why), since they are a function of systems variables such as polarity and the structure of the supplier markets. But for the second wave (1840–1945), Krause highlights the impact of laissez-faire practices on acquisition patterns divorced from the overall facts of political alignments.

By contrast with Krause's work, Laurance focuses on phases of the international system itself and associated variables that can be used to delineate and compare those systems. Hence, various aspects of the arms trade become, in effect, the dependent variables.

Laurance's work begins with an extensive and useful review of the arms trade literature, followed by a section on "conceptualization and measurement of international arms transfers." The latter is the author's forte, as evidenced in previous publications, and might better have been connected to the subsequent analysis of phases in the arms trade. But the core of the analysis involves four successive phases or systems: (1) the *interwar period*, (2) the *postwar bipolar system* (1945–66), (3) the *expanding arms transfer system* (1966–80), and (4) the *declining bipolar system* (1980–92). The 1966–80 period was so demarcated because of the expansion of the number of significant suppliers along with the (primarily) OPEC-fed expansion of the global arms markets—and, on the back end, because of the weakening of the OPEC arms boom, the overall contraction of the market, and the realignments at the close of the Cold War. For sets of independent variables, Laurance relies on K. J. Holsti's scheme for systems analysis: system boundaries, character of system units, the structure and stratification of systems, modes of interaction, and regime norms and rules. These criteria are pegged at a rather high level of abstraction, but Laurance manages to operationalize them by subsuming under them a whole range of real-world problems so as to make them useful for comparisons of his phases.

It is a good effort, although I would question his use of the concept of regimes as applied to the arms trade. This is not easily done (as it has been for, e.g., the problem of nuclear proliferation), because of the absence of both really convergent national attitudes and policies or explicit or implicit rules of the game and any kind of an institutional framework for controls save what has come with the missile technology control regime. I think modesty might have dictated mere reference to a global issue area.

The two books together provide a good framework for analysis of the postwar arms trade and more; and their timing directs attention to a newer phase of the arms trade beyond the Cold War and the U.S. technological exhibition in the Gulf War. Curiously, Krause ends up with the conclusion that we are entering a stage in which the rate of technological innovation in the core has slowed. He says that one consequence is that the gap between the sophistication of weapons possessed by different tiers of suppliers and recipients is narrowing, even as the gap in production capabilities remains extremely wide. By contrast, Pentagon experts are now heralding a military technological revolution claimed to be the most revolutionary since the 1930s, featuring advanced "reconnaissance–strike complexes." The Gulf War is said to represent only the leading edge of this revolution. If true, this has profound implications for the arms trade. Weapons production may fall dramatically in step with lower defense budgets, but the pace of technological change may hasten enough to qualify the present period as the beginning of a fourth "Krausian cycle" of arms production and trade, with the future course of diffusion to new secondary and tertiary tiers open to speculation.

Pennsylvania State University R. E. HARKAVY

Adventures in Chaos: American Intervention for Reform in the Third World. By Douglas J. MacDonald. Cambridge: Harvard University Press, 1992. 361p. $39.95.

Has the United States been a force for progress or an obstacle to reform in the Third World? This is a question that has vexed scholars and policy makers throughout the Cold War. It is likely to remain a central theoretical and policy debate in the years to come. Two basic positions have emerged: some argue that United States intervention will never promote democratization and reform; others suggest that if only the United States adopts the right strategy, it is likely to be successful in this endeavor. If the former position is correct, the United States ought to eschew all Third World intervention; if the latter holds true, it is just a matter of finding the right U.S. policies. What has been lacking until now is a even-handed, judicious, and detailed examination of the conditions under which U.S. efforts to promote reform and stability have succeeded and failed.

MacDonald's book ably fills this lacuna. Beginning with the premise that U.S. intervention is neither always counterproductive nor uniformly beneficial, he closely examines three important cases of U.S. efforts to reform Third World allies. Selecting for variance on the dependent and independent variables, he chooses cases in which U.S. reform strategies and outcomes varied and attempts to establish a correlation between them. The two reform strategies he considers are "bolstering" and "reformist intervention." The former, practiced mostly by Republican administrations during the Cold War, consists of unconditionally supporting a client regime against a military challenge from an internal opponent without demanding substantive change. The latter, the usual policy of Democratic Administrations during the same period of time, involves trying to solve the various political, social, and economic problems that mobilized the opposition to the current government through substantial domestic reordering. His cases involve two failures (American efforts to stabilize the Koumintang government in China and the Diem regime in South Vietnam) and one success (the Magsaysay government in the Philippines). MacDonald's research design seems eminently sound.

In addition, his case studies are extremely well-researched and very informative. They are based in large measure on primary-source documents, which will be read with interest by historians. In addition, they make very interesting side reading beyond their primary analytical purpose of testing the alternative arguments about whether and how U.S. intervention affected these countries. Finally, the cases seem to be reasonable illustrations of his general thesis that reformist intervention is possible under certain circumstances.

It is, however, precisely what circumstances these are that represents one small difficulty with the argument. Specifically, there were so many unique variables in the successful Philippine case that it is hard to tell which were most critical. For instance, was it the long and intimate neo-colonial relationship which made the Philippines more amenable to U.S. influence or was it the fact that unlike China and Vietnam, the Philippines were a series of islands in which the anti-government insurgents could be isolated from external support? To control for all the alternative explanations would require many more cases and degrees of freedom than MacDonald has employed. Given the immensity of that task, one wonders whether it is reasonable to expect any one book to be able to do so. Moreover, while he devotes a whole chapter to discussing the domestic sources of "oscillation" in U.S. policies between "bolstering" and "reformist intervention," MacDonald does not explicitly pose this oscillation, rather than the content of the actual policy, as one alternative explanation for success or failure in the cases. For example, in the two failed cases, U.S. policy shifted back and forth between these two options. In the successful case, however, U.S. policy seemed relatively consistent. In short, my one minor reservation with this very important book is that it has not completely eliminated the alternative explanations for the outcomes.

That criticism not withstanding, MacDonald's book will become one of the most important books on U.S. Cold War efforts to promote political reform in the Third World. Unlike many other books written during the Cold War—and indeed MacDonald explicitly couches his argument in Cold War security terms—*Adventures in Chaos* will have much relevance to post-Cold War debates about U.S. foreign policy. Specifically, while the strategic rationale for U.S. reformist intervention has largely evaporated, the ideological rationale has if anything become more widely heard. With scholars resurrecting Immanuel Kant's classic argument about the peaceful nature of liberal democracies and Georg Hegel's forecast of the arrival of the end of history, interest in the ability of the United States to promote democratic reform around the world has grown. Thus, in addition to policy makers and students of U.S. Cold War foreign policy, those scholars wrestling with the question of what role external actors can play in contemporary transitions to democracy will certainly find much of interest and relevance in this fine work.

University of California, Riverside MICHAEL C. DESCH

Uncertain Perceptions: U.S. Cold War Crisis Decision Making. By Robert B. McCalla. Ann Arbor: University of Michigan Press, 1992. 226p. $34.50.

The role of misperception in foreign policy decision making, particularly during crises, has been a central topic of research for scholars interested in cognitive explanations of international behavior, including classic works by Snyder, Jervis, Holsti, Lebow, George, and others. Robert McCalla's new book on the cold war crisis perceptions of U.S. decision makers contributes to this important line of research. The two major shortcomings of the cognitive approach to foreign policy analysis have been the lack of unifying theory and the difficulty in empirically testing hypotheses about cognitive processes. McCalla seeks to address both of these problems, so I will assess the book in light of these objectives.

The book is organized around five propositions, which form the core of McCalla's theoretical contribution, and five case studies of cold war crises against which the propositions are examined. All of the propositions are grounded in prior work, including the familiar hypotheses that "actors will tend to assume that their opponent(s) see the world in the same way as they themselves see it" and that "decision makers will tend to see crises as the result of deliberate actions by their opponents" (p. 191). In addition, McCalla presents a model of information flow to suggest when preconception will persist and when beliefs will be revised. He distinguishes situational misperceptions, where the actor is misled by faulty information, from dispositional misperceptions, where the actor's own belief systems distort perception. He suggests that unintended crises arising from situational misperceptions will be much easier to defuse since new, more accurate, information should lead to revised perceptions. McCalla further argues that dispositional misperceptions may be a less frequent cause of international crises than is usually supposed. To test the five propositions, he conducts comparative case studies of the Iranian crisis (1946), the Berlin blockade (1948–49), the Berlin Wall crisis (1961), the Cuban Missile Crisis (1962), and the 1973 Middle East War.

Though *Uncertain Perceptions* is very useful on several other dimensions, I do not feel that it takes us very far toward a theory of foreign policy perceptions. Other than distinguishing situational from dispositional misperceptions, very little of the theory is new. And there is little attempt to draw the propositions and the information integration model together into a theoretical whole. Most disturbing to me—and this is a complaint I have with most work in the area—the book displays virtually no awareness of advances in cognitive psychology and behavioral decision theory. These are important bodies of work that any research on decision making should carefully consider. I fear we are unlikely to invent plausible theories of perception and decision making solely from the weak evidence available to foreign policy analysts.

I am also unsure about the degree to which we can disentangle situational and dispositional misperceptions—McCalla's most original contribution. The interpretation of any event or crisis will surely be affected by the quality of information, but it must also be affected by the decision makers' prior beliefs and images. Perhaps faulty information will conjure up the "wrong" images and lead decision makers to unintended crises, but the way the problem is framed depends on how prior, *dispositional,* beliefs are related to the problem. There is also some circularity in the way situational and dispositional misperceptions are operationalized. Situational misperceptions are detected when decision makers prove willing to alter their interpretations of a crisis; but this is also the basic proposition McCalla would like to test. In effect, the operationalized hypothesis must read "when decision makers are willing to change their beliefs, they will be willing to change their beliefs."

Finding empirical evidence that bears on cognitive hypotheses has been another serious problem for foreign policy researchers, so the comparative case studies in this book are a useful addition to the body of case evidence for the importance of beliefs and perceptions. McCalla's case studies are interesting and well-written; they are excellent illustrations of the propositions. Unfortunately, even within the limits of comparative case studies, the results do not support the propositions very strongly. To his credit, McCalla generally resists the temptation to massage the case studies until they fall into line with his theory. But this leaves him with a rather awkward concluding chapter in which he argues that his hypotheses have been generally supported while presenting a table of results that generally belie this conclusion.

Despite these criticisms, *Uncertain Perceptions* is a useful addition to the literature on foreign policy cognition. It does not succeed in revolutionizing the approach, theoretically or empirically, but it does contribute to the cumulation of case evidence. McCalla has done an excellent job illustrating the importance of perceptions in U.S. foreign policy making, though several of his specific hypotheses find little support. Unifying theory, including contact with cognitive psychology and decision theory, and generalizable empirical work remains to be done.

State University of New York, Stony Brook CHARLES S. TABER

The Political Economy of Military Spending in the United States. Edited by Alex Mintz. New York: Routledge, Chapman, and Hall 1992. 334p. $65.00.

Few topics in political economy are more relevant in the 1990s than the politics of U.S. military spending, as budgets are being revamped. Yet political scientists have achieved little consensus on the causes and effects of such spending. Theories of arms races (action–reaction processes) and bureaucratic inertia (organizational processes) have proven almost untestable in the Cold War era because military spending data are too few and of dubious quality. Now increasing attention is being given to political economy theories connecting military spending to domestic politics and economics.

This volume collects 10 studies that use quantitative–empirical methods to analyze some aspect of U.S. military spending (plus three others on related subjects). It is a fine collection of recent work in this genre. However, like many edited works, it fails to synthesize the diverse "findings" represented. Some chapters examine the *effects* of military spending on such variables as welfare spending, economic growth, and corporate profits. Others explore possible *causes* of military spending levels, from election timing to economic conditions and superpower tension levels. Some analyze aggregate military spending, others, spending disaggregated by service branches or functional components.

Data problems plague this research genre. Even that great deductivist, Sherlock Holmes, argued for "data, data, data! You can't make bricks without clay." If the studies in this volume represent bricks to build reliable knowledge about the political economy of U.S. military spending, then almost all need more and better clay. The problems with U.S. and Soviet military spending data are summarized by Kenneth Mayer (chap. 2) and Robin Marra and Charles Ostrom (chap. 13). Soviet spending data are practically incompatible with U.S. data due to problems with exchange rates, purchasing power, and economics of the two military–industrial complexes. United States data on "annual" spending are themselves not a true annual series: a given year's datum reflects overlapping budget cycles (allocations and spending authorizations) that span at least several years from agency planning for budget requests through the final expediture of funds. Long-term movements in spending are evident, but the year-to-year changes are suspect.

Beyond these quality problems is one of quantity. The Cold War produced only 30-odd annual data points at best. Even if these were independent and reliable time-series observations, they could hardly suffice to test models with a half-dozen or more independent variables (typical of the models in this volume). The data shortage is exacerbated in one study by gaps in the main dependent variable, which are filled by manufacturing data points and in two other studies by excluding the 1980s from the analysis because of a hunch that the Reagan years differed from the preceding decades (pp. 56, 188, 220).

The result, in most of the chapters, is too many coefficients chasing too few data points. Regressions with 6 to 8 independent variables but only 32 to 33 cases strain credibility (pp. 58, 125, 190, 252). The resulting high R-squared values and graphs of actual and "predicted" time series are hardly surprising (pp. 59, 127). (The same few data used to estimate the model are then predicted by it.) In the extreme cases, one macroeconomic model has 9 equations and 18 variables but only 27 observations, while one vector autoregression (VAR) model has 48 coefficients and 26 observations (pp. 230, 294). Generally, the "causality" time-series techniques with their included lagged terms (VAR and Granger) cannot produce meaningful results for annual data series this short (see chap. 11–12, 14). The absence of Ns on most of the tables giving regression results is also troubling (pp. 114, 125, 190, 208, 221, 227, 230, 249, 252, 298).

The problems with annual defense spending data *can* be avoided. The best example (which the editor wisely placed at the front) is Mayer's analysis of monthly data on the awarding of defense contracts (chap. 2). The monthly aggregation bypasses the annual spending data and creates a workable N of 394 (even for his 13-variable model). Mayer shows convincingly that contract awards rise just before elections, letting politicians claim credit for the creation of jobs in their districts.

Two other studies avoid the spending variable altogether: Steve Chan analyzes war events' effect on stock prices (chap. 7), and Bruce Russett and Gad Barzilai study causes of Israeli military actions (chap. 8). Both studies, however, use monthly data aggregation and short time periods, leaving only 39 to 69 data points. (Why not weekly aggregation and a longer time frame?)

For annual spending data themselves, pooled cross-sectional and time-series regression would seem the best hope but is unrepresented here. Thomas Cusack analyzes defense spending across 16 Organization for Economic Cooperation and Development countries but only for 1980 (pp. 110–14). Alternatively, one can stick to descriptive methods with multiple data indicators, as Michael Ward and David Davis do for military capabilities, budgets, and corporate profits (chap. 5).

The volume is dated in its references to Cold War issues and trends (especially the speculative concluding chapter). That is not a problem if we can learn something reliable about the political economy of U.S. military spending in that era. The real problem is that although this volume well reflects the state of the genre, most of the studies cannot move from methodological exercises to real-world knowledge generation without more "clay in the bricks."

American University JOSHUA S. GOLDSTEIN

Western Hemisphere Immigration and United States Foreign Policy. Edited by Christopher Mitchell. University Park: Pennsylvania State University Press, 1992. 314p. $45.00 cloth, $14.95 paper.

The title accurately describes the purpose of the book, namely, to analyze how immigration policy toward Western Hemisphere countries is shaped by U.S. foreign policy concerns. Because U.S. attitudes toward specific nations changes from time to time depending on the foreign leaders and the ideological bent of the U.S. administration of the moment, this often gives rise to a double inconsistency in U.S. immigration policy. The policy differentiates between nations (refugees from Cuba are almost invariably treated more cordially than those from Haiti) and toward the same nation at various times (the welcome to Haitian immigrants/refugees varied under the regimes of the two Duvaliers and then changed again after Duvalier *fils* was forced from office). These double differences are the basis for the main recommendation made by Christopher Mitchell, the editor, that U.S. immigration policy ought to be consistent and not slavishly follow the narrow foreign policy criteria of the moment.

That there is an interplay between politics and policy really does not tell us anything we did not know already. Yet the book does a great service by documenting how this interaction has worked in practice. The book contains five policy studies: Cuba, the Dominican Republic, Haiti, Central America, and Mexico. Most of the discussion in the separate chapters is devoted to details of shifts in U.S. immigration policy as country and U.S. leadership changed. The discussion is most penetrating when the analysis gets beyond the simple formulation that politics determines policy or that countries are not all treated alike by the United States.

I found most interesting the two chapters on Cuba and Mexico—the one dominated by Cold War ideology, the other being by far the most salient of the countries sending migrants to the United States. Jorge Dominguez, in the Cuba chapter, elaborates on two central themes. The first is that the "antagonistic cooperation" that existed between Cuba and the United States led each country to choose a migration policy that best suited the interest of the other—for example, the

U.S. acceptance of anti-Castro refugees in the years between 1959 and 1962 (p. 31). The second is that U.S. policy with regard to timing of the sporadic flow of migrants is almost wholly explained by the actions of the Cuban government. The chapter is devoted to documenting these conclusions from nine migration policy episodes.

While foreign policy dominated the U.S. attitude toward immigration from Cuba, which supports the central thesis of the book, Carlos Rico concludes that immigration policy toward Mexico is shaped almost completely by domestic U.S. concerns. In other words, the analysis in this single most important case does not support the central contention of the book that foreign policy considerations are crucial in making U.S. immigration policy.

The key point made by Christopher Mitchell in his chapter on the Dominican Republic is that Dominican migration to the United States paid large dividends to both countries in that it eased social pressures in the Dominican Republic during the first post-Trujillo decade, thereby assisting the Balaguer administration, and in the process drew the Dominican Republic close to the United States. Alex Stepick contrasts the welcome given to the Haitian "airplane migrants to New York" during the Kennedy administration and the more recent exclusion of persons fleeing Haiti (p. 152). He argues that the distinction between political and economic refugees is an ideological one that serves as a rationalization for a policy but has transparent contradictions. He may be correct, but this distinction in the definition of a refugee is not unique to the United States. The determination of who is a refugee has great resonance today in Europe.

Finally, Lars Schoultz differentiates between the formal process of immigration from Central America (under which U.S. consular officers apply universalistic, as opposed current foreign-policy, criteria in issuing or denying immigrant visas) and the granting of refugee status, which is highly politicized. Nicaraguan applications for asylum were clearly treated more tenderly than were applications from Guatemalans or Salvadorans. The Central American case is perhaps the most forceful example of the book's main argument. Schoultz concludes his chapter with the statement that the direction of U.S. policy toward Central America was waged on the battlefield of immigration policy.

Is the case made that U.S. immigration policy with respect to Latin America is thoroughly intertwined with U.S. foreign policy? The answer is evidently *yes* when it comes to refugee policy. The conclusion is much more ambiguous when it comes to formal immigration, as Schoultz makes clear in his discussion. However, the central premise of the book breaks down when it comes to the formulation of U.S. immigration policy toward Mexico. This is a monstrously big exception.

The book is strongest in the detail of the case studies. These lay out, more completely than I have seen elsewhere, the reasons for the differentiated immigration policies toward the separate countries at particular periods depending on what was going on in the United States and in the countries themselves. The cross pressures in the United States and in the countries are well integrated by each of the authors. The book is less strong in providing remedies. The conclusion that immigration policy does not work well as a foreign policy tool and, in the process, often carries a high price for individual migrants and for the United States as a moral community is well documented in two of the case studies, those on Haiti and Central America. Mitchell's conclusion on the Dominican case seems to be that immigration policy there did have a positive foreign policy outcome. Cuba and Mexico are each sui generis. Furthermore, it is unclear that the suggestion to separate the two sets of concerns will effectively divorce two policies that often have many of the attributes of Siamese twins with a single heart. In addition to the recommendation for more consistency in formulating U.S. immigration policy, Mitchell makes a plea for more humane treatment of migrants who are detained, which, while desirable, does not get at the central issue of the book. He also tentatively suggests that the creation of a new migration commission could raise the salience of this issue in policy planning. When all else fails, reorganize.

University of Texas, Austin SIDNEY WEINTRAUB

Economic Discrimination and Political Exchange: World Political Economy in the 1930s and 1980s. By Kenneth A. Oye. Princeton: Princeton University Press, 1992. 235p. $35.00.

Cooperation and Governance in International Trade: The Strategic Organizational Approach. By Beth V. Yarbrough and Robert M. Yarbrough. Princeton: Princeton University Press, 1992. 182p. $35.00.

In the last two decades, the non-discrimination norm has come under challenge from bilateralism, regionalism, and conditional most-favored nation agreements. Under what conditions are these arrangements discriminatory or liberalizing? Working along quite different theoretical tracks Kenneth Oye and Beth and Robert Yarbrough have produced two excellent analyses of this crucial issue.

The Yarbroughs state at the outset that they are less interested in the determinants of liberalization per se than in the form it takes: unilateral, multilateral, bilateral or minilateral. To explain this variation in governance structures, they turn to the literature on transactions costs, and in particular, to work on the contractual management of opportunism in markets characterized by asset specificity. When assets are dedicated to a particular purpose or relationship, then the investor runs the risk of being "held up" by her purchasers. How can such risk be managed?

The Yarbroughs argue that governance structures are determined by the extent of relation-specific trade in the system and the availability of third party enforcement. In the absence of dedicated assets, parties can determine their trade policies unilaterally. They argue that though Britain retained an interest in opening markets abroad, the country faced few risks from opportunism and could liberalize unilaterally.

As new products entered world trade, the potential for opportunism increased. One organizational option is a hegemon capable of wielding the carrots and sticks of enforcement; the Yarbroughs draw on and extend public goods interpretations of hegemonic stability theory. But in the absence of a hegemon, we would expect other governance structures to emerge. The Yarbroughs explore bilateralism as a response to limited third party enforcement, and examine the techniques for making bilateralism stable and self-reinforcing. These include mutual hostage-taking (for example, through negotiation of relation-specific standards) and bundling goods into a single agreement so that the costs of termination outweigh the gains from non-compliance. "Minilateral" agreements are a final organizational option, creating supranational institutions that provide third party enforcement.

The Yarbroughs' analysis raises some of the same questions as those posed to the new institutional economics on which they draw. First, as the Yarboroughs recognize, it is difficult to gauge the extent of relation-specific investment and the depth of concern about opportunism. They argue that the growth of intra-firm trade is an indicator of the growth of dedicated assets, but the international vertical integration implied in such trade already provides a governance structure that is equally if not more important than the government-to-government contracts that are the focus of the study. An extension of the research agenda opened by this book will be to investigate the trade policy preferences of sectors and firms to gauge the extent to which it is driven by the concerns that they hypothesize.

A second empirical problem is the coexistence of multiple governance structures. The Yarbroughs' two-by-two matrices distinguish among systems on the basis of the effectiveness of third party enforcement and the existence of specific trade-related investment. But the current international system combines trade that fits in all cells. This might explain why we see the concurrence of unilateral liberalization (on the part of a number of developing countries), ongoing multilateral discussions (the Uruguay Round), minilateral experiments (APEC, NAFTA, etc.), as well as an explosion of bilateral agreements. Yet the precise relationship between these organizational forms and relation-specific trade is not always sharply drawn.

Two further theoretical issues deserve comment. First, the characterization of the hegemon as providing third party enforcement is not altogether an accurate picture of the period of American dominance. The U.S. enforced compliance in cases that affected its interests directly, but not others; third party enforcement remained an underprovided public good, particularly for small countries. This is related to a second point. Many bilateral arrangements are stable not because of the mechanisms outlined by the Yarbroughs, but because they involve the exploitation of power asymmetries.

Oye's book is an exploration of the conditions under which bilateralism, or what he calls "unrestricted bargaining," is likely to be liberalizing. The book is theoretically complex, but the central claims are straightforward. If strong liberal rules exist that already constitute an effective check on opportunism, then bilateralism is likely to be discriminatory. In situations where rules are breaking down rapidly, as in the 1930s, then commitment to multilateralism can make things worse and unrestricted bargaining can both stop the slide and serve as a focal point for the reconstruction of multilateral relations. Second, it makes a difference if bilateral bargains are based on exchange and reciprocity as opposed to extortion; the former is good, the latter is bad. Finally, a more general argument is that bilateralism assists the cause of liberalism because it clarifies the gains from exchange and thus provides an incentive for liberalizing forces to overcome collective action problems at the domestic level.

Oye examines trade and finance in two periods, the 1930s and the 1980s. The empirical analysis of monetary relations in the 1930s is probably the most original of the four cases, but the findings with reference to trade are the most relevant for current debates about the future of the multilateral system. Oye argues that we should not draw false analogies to the 1930s because policymakers in the earlier period held preferences and economic beliefs that were dysfunctional for the management of the international economy. This is not inconsequential, but I would give greater weight to different economic circumstances, namely the depth of the Great Depression and the much greater market interdependence that characterizes the current period. As Helen Milner argues, the increasing internationalization of the world economy provides an important check on illiberal policies; this argument can be extended to explain limits on "bad" bilateralism and regionalism as well. The most internationalized corporations are the champions of the liberalizing bilateralism and regionalism that flourished in the 1980s, from the deepening of the EC to the new strategic trade policy.

Nonetheless, Oye is correct in noting that bilateral bargaining provided the only plausible exit from the closure of the interwar period. His argument is also convincing that the apparent hypocrisy of the current system should not be a cause for alarm, since the existence of rules against non-discrimination has had the effect of constraining "bad" bilateralism while allowing "good" bilateralism and regionalism to flourish.

The difficulties with Oye's analysis are less with substance than style. Oye makes contributions to the theory of externalities, offers a useful tripartite distinction between three forms of issue linkage (exchange, extortion and explanation), examines the effect of bilateral bargaining on domestic preferences, and digresses on the role of beliefs and different informational assumptions. Yet this theoretical richness sometimes obscures the book's core observations.

Despite these criticisms, these are two important books on international political economy. The Yarbrough's book has a crisp theoretical structure drawn from the transactions costs framework and opens a variety of new avenues for empirical research. Oye makes contributions to a number of debates in international relations theory; his concept of "divertible" externalities and discussion of different forms of issue-linkage will be widely used. Interestingly, the books come to surprisingly similar conclusions: that the trend away from multilateralism poses fewer risks to the international political economy than purists might think.

University of California, San Diego STEPHAN HAGGARD

High-Tech Europe: The Politics of International Cooperation. By Wayne Sandholtz. Berkeley: University of California Press, 1992. 360p. $39.95.

International High-Technology Competition. By F. M. Scherer. Cambridge: Harvard University Press, 1992. 196p. $29.95.

Who's Bashing Whom? Trade Conflict in High-Technology Industries. By Laura D'Andrea Tyson. Washington: Institute for International Economics, 1992. 352p. $40.00 cloth, $25.00 paper.

Technology policy, according to Inman and Burton, is an essential component of a "New Policy Frontier" (*Foreign Affairs* 69, no. 2 [1990]:). Even though they do not specify the contours of technology policy, Inman and Burton maintain that the United States needs a new policy framework to promote the technological prowess of the American economy and address the issues of international competitiveness.

Reams of reports and analyses in the last half-decade have sought to contribute to this policy framework, and countless elected officials and aspirants have presented technology initiatives as if they were this decade's counterpart to the "supply-side" proposals of the 1980s. While such public attention can sometimes corrode the quality of scholarship, the three volumes reviewed here provide reassuringly sound analyses of the issues they examine.

Each of these volumes investigates how established institutions are adjusting to the challenges raised by international competition in high-technology industries. Scherer examines why individual U.S. firms adopted the strategies they did when their products faced competition from technically advanced imports. Sandholtz assesses the factors that led European nations to respond to the perceived weaknesses of their high-tech industries with cross-national research-and-development programs such as ESPRIT and EUREKA. Tyson considers how the United States should react to Japanese and European policies to promote the interests of their high-technology producers. Each of these mainstream works regards technology policy as one factor, among a number of others, that influences how effectively a country will respond to the economic challenges of the future.

Scherer concentrates on the private sector and strives "to illuminate empirically how real-world business enterprises interact in an environment of international competition characterized by technological innovation, product differentiation, economies of scale, and the strategic quest for comparative advantage" (p. 23).

The book presents three perspectives on these issues. The first is a review, with occasional commentaries, of the implications of economic models for competition in high-tech industries. The second element of the book, the longest, summarizes 11 case studies of how U.S. firms responded to product innovations brought to market by international firms. The final chapters report the results of two separate regression analyses of trade, import penetration, and corporate variables for large samples of U.S. firms. Three elements of the book inform each other, but they do not create the synergistic perspective the book's design seeks.

Scherer acknowledges this point. The theoretical discussion, he writes, "yields ambiguous predictions" (p. 45). The case studies are "difficult to generalize," and the regression analyses are based on attenuated operationalizations and generate results that have "weak explanatory power" (pp. 113, 151, 185). Noting that his research was meant to be "descriptive," rather than perspective or theoretical, Scherer correctly writes that the "research reported here . . . is by no means the last word on the subject" (pp. 181, 189).

Despite these qualifications, significant value comes from sharing the interim insights of a talented researcher struggling with an inchoate area. Scherer believes that the analysis of high-tech industries poses distinct problems. Citing Schumpeter, he maintains that "technological innovation is more likely to be characterized by imperfect information, recognition lags, and disequilibrium than by the primitive but stringent competitive assumptions" that underpin neoclassical, equilibrium-oriented economic analyses (p. 27). For Scherer, the features of the product technology interact with a firm's organizational characteristics to shape that firm's response to the challenges presented from international competitors.

Scherer emphasizes the diversity of corporate reactions to these challenges. From the case studies, he categorizes the responses of U.S. firms as submissive (surrendering the market), aggressive (increasing efforts to meet the challenge), or focused (continuing vigorous R&D efforts but ceding sizable market niches) and then seeks to explain why the firms adopted these strategies. Although the data are rarely persuasive, Scherer finds that large producers, businesses in concentrated markets, firms with diverse product lines, and companies with technically trained leaders have reacted more aggressively to international challenges. He also concludes that firms benefiting from trade protection have usually responded less effectively to international competition than those that stood alone.

Sandholtz echoes the view that high-tech industries possess distinctive characteristics; but what is important for him is that these industries became a strategic priority for European governments in the mid-1980s. While these countries had devised "national-champion" policies to promote their lagging, technology-oriented industries in the 1960s, Sandholtz seeks to discover why they turned to cooperative R&D programs in the 1980s.

Sandholtz's analysis is based on reviews of the ESPRIT, RACE, and EUREKA programs and is supplemented with comparisons to Euratom, Unidata, Airbus, the European Space Agency, and EC technology proposals from the 1970s. Sandholtz explains the emergence of cooperative European R&D programs in the 1980s on the basis of "cognitive variables" (i.e., the recognition that national champion policies had not worked), policy leadership from prominent firms and the European Community, and proposals that recognized the cost–benefit calculations of important actors. Sandholtz endorses the view that the recent European R&D programs were successful because they promoted intra-European cooperation among European firms.

High-Tech Europe is written from the perspective of the European Community and provides an especially informative account of the origins of ESPRIT. The examinations of RACE and EUREKA are also more useful than those found elsewhere and more comprehensive than the case studies relied on by Scherer. Little attention, in contrast, is directed toward the national technology programs that remain far larger than the European efforts or to the record of corporate joint ventures that was already being written before the European programs were launched. It is problematic to judge the significance of the European R&D process without assessing parallel R&D activities and without evaluating the capacity of the European projects to achieve their technological objectives. Alternative interpretations of the dynamics of European R&D programs receive little scrutiny.

Tyson's premise is that technology-intensive industries violate both the assumptions of free-trade theory and the static concepts on which much economic analysis rests. The core of the book is a set of case studies of how international competition in computers, cellular telephones, semiconductors, commercial aircraft, and consumer electronics is distorted by the practices of foreign governments and firms. Drawing upon these cases and recent developments in trade theory, Tyson argues that a nation's competitive position in these industries is less a function of factor endowments than of national and corporate policies. The United States, she contends, can no longer afford to ignore the efforts of Japan and Europe to promote their high-technology producers at the expense of American industries.

Who's Bashing Whom? is a superbly argued, even courageous, book. Tyson stands in opposition to much equilibrium-based macroeconomic analysis. She shares with Scherer and Sandholtz the view that technology issues pose new analytic problems, but she then relentlessly pursues the logic of this position. She, too, relies on case studies to advance her argument; but she repeatedly engages, and builds upon, the relevant analytic literatures. She excels at using the case material to fortify her positions and amplify her conceptualizations and those of others.

Who's Bashing Whom? will alter the terms of policy debates about trade, competitiveness, and technology. Tyson describes convincingly the impediments to the international flow of high-tech goods, but equally persuasively warns of the drawbacks of voluntary export restraints, antidumping sanctions, and similar trade-restricting measures. She insists that flawed domestic choices are the fundamental cause of U.S. economic problems but concentrates her attention on devising policy tools that will encourage trade and competition and appears to favor subsidies for U.S. high-tech firms that must compete with industries assisted by international policies, practices, and structures.

Despite its subtitle, *Who's Bashing Whom?* may have as much significance for domestic policy as for trade policy. The "fate of the nation's high-technology industries," Tyson concludes, "depends not on the trade battles we fight abroad but on the choices we make at home: in macroeconomic policy, education policy, technology policy, and industrial policy" (p. 296). Rather than viewing these policy domains as alternative areas of emphasis, Tyson argues that success in international high-tech industries requires a more effective complementarity among policy domains. She attaches particular importance to the structure of economic activity in Japan. Readers may not accept all of her positions, but Tyson forces us to overhaul how we think about a set of critical issues.

In the late 1960s and 1970s nonpartisan academics demonstrated that established structures for regulating industries had lost some of their effectiveness as tools for securing the country's traditional policy objectives. In the next decade, these arguments were kidnapped and transformed into broadbrush attacks on government regulation. Tyson's argument is so effective that it, too, could be captured and impressed to support the particularistic agendas of industry lobbyists and quick-fix, elected officials.

Students of technology issues have generally accepted the analytic premises of neoclassical economics and deferred to the policy recommendations of traditional economists. Each of the volumes reviewed here demonstrates that neoclassic economic premises are being challenged in examinations of technology themes. Curiously, few political scientists have investigated the policy implications of new technologies as profoundly as Tyson or sought to address those issues in a policy framework that acknowledges contemporary assumptions. In the absence of such scholarship, the invitation to those pursuing private agendas to misuse Tyson's analysis and the other research reviewed here remains open.

Rutgers University RICHARD LEHNE

The Highest Stakes: The Economic Foundations of the Next Security System. By Wayne Sandholtz, Michael Borrus, John Zysman, Ken Conca, Jay Stowsky, Steven Vogel, and Steve Weber. New York: Oxford University Press, 1992. 262p. $29.95.

Over the past decade, the Berkeley Roundtable on the International Economy (BRIE) has played a leading role in the public debate over industrial policy. Voices in the wilderness during the Reagan–Bush era, its members are now playing a prominent role in the Clinton administration, as former BRIE associate Laura D'Andrea Tyson has assumed the chairmanship of the Council of Economic Advisers. If for no other reason than to gain a deeper understanding of the ideas now in play in Washington, *The Highest Stakes*, which brings together six BRIE working papers written over the past several years, should be required reading.

The basic BRIE argument is that "manufacturing matters" to both the wealth and power of industrial nations. The authors suggest that American decline is mainly a function of its loss of industrial competitiveness, while the relative rise of Japan and Western Europe is largely due to policies that promoted a diverse and dynamic industrial base. Inasmuch as industry, particularly high technology industry, provides the basis for both economic and military security, it is imperative that nations maintain needed capabilities at home and intervene in the economy when necessary in order to prevent "competitive dependence"—dependence on foreign suppliers for the "components, materials, and equipment" that domestic companies need in order to increase their productivity. It is not an adequate response, they argue, to allow "the market" to engineer a shift from an industrial- to a service-based economy; services cannot stand on their own, since they rely on industry for the bulk of their business and, in any event, are insufficient to create and maintain wealth and power.

With the end of the Cold War, BRIE argues, it is even more imperative for the United States to respond to its relative industrial decline. During the period when the Soviet Union threatened the West, the overwhelming mutual security concerns of the allies outweighed their economic disputes and made possible the continuation of an international economy with the United States at its helm. Today, in the absence of a common threat, countries feel less constrained in their economic policies and more willing to risk policy disputes with Washington. In the event, to the degree that the end of the Cold War means American military withdrawal from its forward-deployed positions, additional incentives exist for the Europeans and Japanese to create regional economic-security systems.

Gazing toward the future, the BRIE researchers see three possible international structures: (1) managed multilateralism, which is basically a continuation of the postwar model but with leadership provided by "the three" instead of by an American hegemon; (2) benign regionalism, in which the United States, Japan, and the European Community, while becoming increasingly bloc-oriented, are able to continue trade and investment relations, albeit at lower levels than at present; and (3) twenty-first-century mercantilism, characterized by "a regional rivalry . . . where the drive for autarky is fueled not by welfare concerns but by worries about relative position and competing state power" (p. 175). While refusing to give an outright prediction, they argue, "Most likely we face a choice between some version of managed multilateralism and twenty-first century mercantilism" (p. 175). The ultimate choice will be determined not only by relative economic-growth rates and changing security concerns but also by domestic interests within each of the three regions.

What is to be made of this argument? The question is hard to answer, since the academic reader can only finish *The Highest Stakes* wishing it had provided a more powerful explication. While the chapters, covering industrial policy in America, Japan, Europe, and the Third World, provide stimulating food for thought, we get something less than a meal. The authors frequently rely on assertions that are not supported by data and arguments whose causal connections are often unclear. Further, some of the chapters were obviously written in the mid-1980s. Neither Japan's economic problems nor the relative rise of China, for example, are treated in the text. Indeed, in the conclusion, Steve Weber and John Zysman do not even discuss Chinese views toward a regional economy that centers around Japan, and they underplay the political problems that exist in the Asia–Pacific region. The same may be said of the chapter on the European Community (by Wayne Sandholtz and John Zysman); between the Yugoslavia crisis, German reunification, and the continent's chronic economic woes, the Community is now looking like a rather feeble global player.

The authors also fail to draw out the policy conclusions of their findings. They are very taken by strategic trade theory; but, as with that theory itself, they find it difficult to deliver specific recommendations to public officials. While their argument rejects the free-trade ideology that has captured Washington over the past 40 years (in theory if not always in practice), they do not really provide an alternative framework. As presently articulated, their argument could easily be captured by protectionist interests from almost every domestic industry.

Despite these reservations, *The Highest Stakes* deserves to be read by political scientists and public officials. It raises important questions about the future of the international economic system and indicates some of the alternative structures that may emerge. Further, it links economic and security concerns in an insightful fashion. But it fails to address the one possibility that is perhaps most likely of all—continued American dominance of an international system that lacks any serious challenger.

Brandeis University,
Harvard University ETHAN B. KAPSTEIN

Justice and World Order: A Philosophical Inquiry. By Janna Thompson. New York: Routledge, Chapman & Hall, 1992. 211p. $69.95 cloth, $16.95 paper.

The premise of this book is that there will continue to be serious disagreement about principles of international justice. Rather than discussing such principles directly, it is therefore more fruitful to approach questions of international justice by imagining changes in social relations, motivations, and forms of political authority that might encourage international actors to cooperate despite their continuing disagreement. Thompson also wishes to identify mechanisms that might move us beyond an international scheme of self-interested cooperation, or 'modus vivendi,' to a genuine community of shared values. She argues that neither realists nor Marxists have shown that a gradual, peaceful transition to a more just international world is impossible. She then imitates Kant in *Perpetual Peace* (1795) by telling various stories about how alternative kinds of international society might arise. The purpose of these conjectural histories is to link questions about the desirability of alternative world orders to questions about their practical feasibility. It is also to give us hope that our attempt to act ethically in international relations may not prove in vain.

In the first half of this book, Thompson's target is cosmopolitan theories of international justice that take individual rights as their ultimate concern. The forms of world order most often associated with such theories are a world federation of states and a world state. Thompson argues that a world federation of states might emerge from the development of various regional states, such as a united Europe, but she questions whether such super-states would be sufficiently motivated to behave justly toward one another. Alternatively, a world state might emerge from a highly integrated world economy, but Thompson doubts whether a global state could generate the kind of communal feelings necessary to motivate compliance with duties of global distributive justice to individuals. In general, Thompson subscribes to the communitarian critique of cosmopolitan visions of world order: such visions neglect the ways in which identification with local communities provides individuals with meaning, with strong motivations to help their fellow members, and with a means of reconciling the demands of their public and private lives.

In the second half of this book, however, Thompson also embraces the liberal critique of communitarianism: for many kinds of communities (including the territorial state), 'belonging' often involves an unacceptable sacrifice of individual rationality and freedom. To make this point, she constructs conjectural histories of how a world of small, democratic communities, or alternatively, a world of closed nation-states might emerge. Given liberal objections to these forms of community, what is required is a new form of association that provides both belonging *and* freedom.

Thompson's answer is a new conception of nationalism, where the 'nation' is composed of many open communities whose membership and objectives overlap. This overlapping membership allows individuals both to belong to and exit from many different groups, so that no single group or category of person can easily be identified as an enemy, and so that communities interlock in a way that disposes them to value common methods of dispute resolution even in the absence of fully shared goals. These 'nations' are not territorial nation-states, however, but criss-crossing trans-state associations that negotiate agreements with more traditional territorial states that also continue to exist. Thompson argues that this kind of pluralistic, interlocking global society of new 'nations' and older nation-states would tend to be more peaceful and just than any other form of world order.

Of course, this sort of global development has been imagined before. In *The Anarchical Society* (1977), for example, Hedley Bull described something like it under the rubric of 'neo-medievalism.' One critical question that needs more discussion is whether such an order really would be more peaceful. After all, in this kind of international society, traditional state-centered international law has been replaced by a series of negotiated agreements between different kinds of communities which contest each other's authority. Another question is how this or any of the other forms of international society that Thompson discusses could serve as a concrete ethical ideal to work for, since all of her alternative histories involve a long course of evolution, in some cases spanning several centuries. A related issue concerns not only how we get from here to there, but what it is permissible to do in the process. This issue is only addressed in the vaguest of terms, e.g., we are told that we should try to make the associations to which we already belong more just, and that we should not use violence to create a new order, although "this does not mean we should accept Kant's prohibition against revolution or wars of national liberation" (p. 192). One would have thought that any discussion of justice that stresses its own practical character would have to consider these sorts of issues in more detail.

Having registered these criticisms, however, it should also be noted that some of this vagueness simply comes along with Thompson's particular method of approaching her subject. That method, which focuses on institutional change rather than philosophical principle, is both original and interesting. Thompson's book also contains imaginative discussions of Rousseau, Kant, Hegel, Marx, Rawls and other major political theorists on international relations, presented in a clear and unpretentious style.

University of Colorado, Boulder David R. Mapel

Gender in International Relations: Feminist Perspectives on Achieving Global Security. By J. Ann Tickner. New York: Columbia University Press, 1992. 180p. $30.00.

Throughout the 1980s feminist analysts steadily built an alternative literature of international relations that critiqued the field's many unacknowledged gender presumptions and showed how the study of war, peace, development, and other topics would change if we took women's perspectives into account. Now, nearly 10 years into feminist international relations, there is growing interest in displaying this virtuosity to students. Ann Tickner's volume is a prototype of a feminist analysis that brings theory critique and reformulated issue concepts into an accessible introduction to international relations from the other side.

Tickner's stated aim is not to produce a women's international relations so much as to integrate feminist perspectives into the field as a first step on the road to a new international relations that can transcend gender altogether. Her focal point for the study—security—is an apt vehicle for the journey. It rivets our attention on one of the traditional core concerns of the field and then strips the security core naked so that we can see its masculine-serving power guises. All gussied up with holsters bristling nuclear weapons, spitoons ready to catch the waste (maybe), and a garbled lingo of games and threats, realist and neorealist writings on security, it seems to me, continually restage the oft-caricatured OK Corral scene in Tombstone, Arizona where believers now gather to hear soulful renditions of a shoot-out in a lawless place. Tickner refuses to cheer on the national sidelines while wary states square off in dangerous but apparently delicious contests dictated by a pregiven anarchic order. She also refuses rationally played games of cooperation under anarchy that incorporate so many gendered assumptions that she doubts whether women "would have selected this methodology for explaining the behavior of states in the international economic system" (p. 83). Tickner, however, does not merely refuse. She provides readers with numerous opportunities to see the gendered aspects of an entire field and to conceptualize alternative starting points that make more sense for all, and not just some, of us.

After critiquing security in its mainstream forms, focusing on the work of Hans Morgenthau and Kenneth Waltz, she extends the analysis into the realms of international political economy and international environmental concerns. To some, this elaboration of security and where we should study it might blaspheme aplenty. But Tickner delightfully compounds the "sin" by insisting that in each component realm of security, the action truly worth studying is occurring beneath our usual level of vision. That is, if we want to understand national security, we should look at the hierarchy of gender roles that dictate what is, and who may and may not play, the soldier on whose behalf. If we want to understand our need to lionize Great Powers, we should look first at gender hierarchies within western societies, where metaphors of strong and weak, greater and lesser, civilized and savage are created. If we want to get our histories right, we should turn back the pages of international relations to unsung moments, such as that of Sacajawea, a Shosone tribeswoman married to a French interpreter in the northwest, whose presence at meetings of explorers and Native Americans had the effect of encouraging peace, "since the Native Americans assumed that war parties would not include women" (p. 63). If we want to grasp economic security or consider the downsides of economic nationalist prescriptions, we should talk to women in third world debtor countries. If environmental security is the puzzle at hand, we might consider the feminist literature that draws linkages between commonplace understandings of the earth as the quintessential female and national policies toward women. On it goes. Tickner tirelessly dislodges the premises of security that perpetuate stories of well-fortified heroic states at the ready to foil (inevitable) relative gains-seekers.

Yet the book has flaws. At the simplest level, it tickles the fancy and moves on too quickly. Admirably concise, it is perhaps too short, too frustratingly suggestive. Just as one has wrapped the mind around a tantalizing possibility for rethinking international relations, Tickner is on to a new topic or angle. More problematic is Tickner's tendency to lose her way a bit as she tries simultaneously to grapple with crowded feminist theoretical terrains and to offer strong feminist recommendations for international relations. Consider this recommendation: "Any feminist definition of security must therefore include the elimination of all types of violence, including violence produced by gender relations of domination and subordination" (p. 58). Some readers will cheer such a conclusion. But what of liberal feminists who seek not only to enlist in the U.S. military, but to be combat trained and combat experienced as a way of ending the domination of the armed services by men? What of Sacajawea's role in bringing Native Americans into a violent history of other-denying peace? What also of people called women who do not match Tickner's sense of them, the ones who, for instance, feel smothered by ongoing connections and relationships and seek to defect? What would they make of this statement: "Ecology's emphasis on holism and reproduction and metaphors such as housekeeping connect it to women's rather than men's life experiences" (p. 98)? We might ask which women connect to holism, reproduction, and housekeeping and which ones resist some of these connections.

There is one other key question to raise in relation to Tickner's book: Is the point to learn from the variegations of feminist theory or to take lessons for international relations from particular women's lives? Feminism addresses women, but increasingly it problematizes women as "women." Tickner is nearly unfaltering in her certainty that real women exist; that women have life experiences to match typical tropes; and that women have uncolonized wisdom to tap for international relations. All of this may be somewhat true. But it is also the case that "women" is an assignment of realms, tasks, and powers that some embrace, others reject, and still others embrace-reject-amplify-transsexually-combine and cross-culturally construct differently. Despite the postmodern turn in feminist theorizing, Tickner nods only briefly to the problematic of "women" before heading off on her standpoint steed to find and report on women's ways.

These are critiques from within feminist theory. Tickner's book aims to provide a first cut at gender in mainstream theories of international relations. IR texts are usually heavy with a masculinist baggage that defies any objective style of presentation. Under the weight of tradition, though, students can be without words to express their sense that something-somebody may be missing. Tickner's book provides ways to begin to frame discomfort with this narrowly gender-conceived and yet oddly self-satisfied field. It features the new and the bold and the uninvestigated. It provides alternative points of departure for theory and impresses us with the amount of work feminist scholars have already done to clear the brush. Of utmost value, it tells of the many ways the field of international relations needs feminist thinking to get its knowledge and priorities straight.

Northern Arizona University CHRISTINE SYLVESTER

Cooperation and Discord in U.S.-Soviet Arms Control. By Steve Weber. Princeton: Princeton University Press, 1991. 331p. $37.50.

One of the most consuming puzzles in the study of international relations is why we see self-interested nations (in particular, the superpowers during the Cold War) cooperate on some issues. Steve Weber addresses this puzzle by focussing on U.S.-Soviet arms control, and asking "[w]hy have the United States and Soviet Union achieved cooperative agreements with the goal of enhancing their mutual security in certain arms control issues, but not others?" (p. 3) In particular, he examines three cases over which the U.S. and Soviet Union have tried to reach an agreement on arms limitation: antiballistic missile systems (ABM), multiple independently targetable reentry vehicles (MIRV), and antisatellite weapons (ASAT). These three cases represent three distinct outcomes: the successful signing of an ABM treaty in 1972, the failure to form a cooperative agreement with regard to MIRVs, and the initial (but tacit) success at limiting ASATs until the 1980s when the U.S. chose to move forward in ASAT development.

To explain these diverse outcomes, Weber borrows from the game-theoretic literature on the iterated prisoner's dilemma (IPD)—primarily from Robert Axelrod's *The Evolution of Cooperation*. Weber claims that the formal work on the IPD has shown how cooperation can come about through the use of reciprocal strategies such as tit-for-tat (TFT), but cannot explain why there may also be cases where cooperation does not prevail. Weber concludes that the formal literature is not adequate to explain failures as well as successes. Leaving this dubious claim aside for the moment, we will look at how Weber chooses to alter the theory to make it applicable to the specific area of arms control.

To do this Weber develops a set of "bridging principles" whereby he ties the formal model to his historical cases. Weber uses the first three chapters to argue that the cases he has chosen are appropriately modeled by the IPD framework, but on the other hand he argues that the theory is not elaborate enough to explain specific empirical examples, such as arms control. In order to "extend" Axelrod's framework to incorporate arms control Weber borrows themes from a wide range of other theories, such as prospect theory and cybernetic decision making theory, both of whose principles violate many of the assumptions underlying the game-theoretic literature on which Axelrod's study is based. This exercise in theory-building is not sufficiently coherent, leaving the reader unsure whether the composite extends Axelrod's framework or results in self-contradiction.

Weber focuses on strategies for an explanation as to why there are different outcomes. He proposes that there are different types of reciprocal strategies, some of which are better at eliciting cooperation than others. Weber's typology of reciprocal strategies is based "on the criteria of *contingency* that delineate when and what kind of retaliation is to be delivered in response to a 'defection' on the part of the adversary" (p. 54). Which strategy is adopted by a superpower is determined by a set of five structural variables: (1) interests and incentives, (2) saliencies, (3) the general political environment of U.S.-Soviet relations, (4) strategic theory, and (5) bureaucratic pressures.

These five variables are really the heart of what Weber contributes to the explanation of arms control outcomes, yet his discussion of these factors remains curiously minimal. Reading through the individual case studies in chapters 4 through 6, it is clear that Weber believes that the influences of these five variables are the deciding factors in whether or not cooperation is achieved. They influence more than just strategies, but also preferences, which could potentially change the structure of payoffs away from a PD game. Unfortunately, Weber gives us no argument for how these variables systematically affect the arms control process. A future productive line of research would be to build a theory about the relationship between these variables and how agreements come about. From this theory, the conditions under which to expect success and failure in cooperation could be determined and consequently tested for in various empirical cases of arms control.

Weber starts off with an interesting puzzle as the focus of his research, but falls short of developing an adequate theory when he decides to force it into Axelrod's framework instead of developing and testing his own ideas. Contrary to what Weber claims, the IPD framework *can* explain failures to cooperate—for example, the ecology of strategies could be such that TFT and other strategies that elicit cooperation cannot succeed, and defection is the dominant outcome. But this is clearly an explanation that cannot be applied to arms control because there are only two nations interacting. Instead of recognizing that Axelrod's IPD model is inappropriate for his empirical interests, Weber engages in a rhetorical exercise to convince the reader that certain issues in arms control can be looked at using the inappropriate IPD framework. When Axelrod's IPD framework is clearly inadequate, Weber attaches ideas from other theories which then make his theory internally inconsistent.

There are clearly many interesting avenues to explore regarding the role of Weber's structural variables and the possibility of cooperation in arms control. To accomplish this, Axelrod's IPD framework could be set aside, and a theory more appropriate to arms control developed.

Michigan State University GRETCHEN HOWER

Deterrence and Strategic Culture: Chinese–American Confrontations, 1949–1958. By Shu Guang Zhang. Ithaca: Cornell University Press, 1993. 302p. $37.95.

Traditional studies of deterrence have frequently suffered from one of two problems: (1) excessive theoretical abstraction or (2) incomplete treatment of the bilateral nature of a deterrent relationship. Shu Guang Zhang's *Deterrence and Strategic Culture* is an attempt to avoid these two problems by looking carefully at both sides of a deterrent relationship, in this case the U.S.–Chinese relationship from 1949 to 1958. Drawing on newly available materials that illuminate Chinese policy, as well as recently declassified U.S. documents, Zhang examines seven "mutual deterrence" situations: (1) efforts by Chinese leaders in 1949–50 to deter U.S. intervention against the communists and the Truman administration's calculations about such intervention, (2) Truman's dispatch of the Seventh Fleet in June 1950 to the Taiwan Strait and Beijing's response, (3) the crossing of the thirty-eighth parallel by UN/U.S. forces and Chinese intervention, (4) the endgame bargaining in the Korean War, (5) the possibility of American and Chinese intervention during the Indochina crisis of 1954, and (6-7) the First and Second Taiwan Straits crises in 1954–55 and 1958 (pp. 11–12). The rich historical detail will be of particular use to those interested in early U.S.–Chinese relations, while the theoretical framework draws attention to underlying assumptions of parts of the deterrence literature and raises questions about the role of *culture-bound perceptions* in deterrent situations.

Deterrence and Strategic Culture has the same blend of theoretical analysis and historical research found elsewhere in the Cornell Studies in Security Affairs Series (e.g., Posen's *Sources of Military Doctrine* [1984] and Brown's *Flying Blind* [1992]). As Zhang notes, his work "does not aim at deducing concepts, or theories from historical 'illustrations', but attempts to depict the problem of deterrence as much as possible on the basis of detailed historical evidence" (p. 9). The discussion of deterrence theory and previous efforts to understand historical examples of deterrent behavior is brief, setting the stage for the heart of the book, which is the historical accounts. Zhang argues that traditional deterrence theory, with its usual assumptions of an aggressor and a defender, focus on single instances of deterrence, and lack of attention to the mixed means that states can use to achieve deterrence, is inadequate to explain the behavior of the United States and China in these cases. By carefully examining both sides of the deterrent equation, with particular attention to how each actor viewed the crisis and the motives and actions of the other, Zhang clearly outlines the interaction of perceptions and behavior that drove these crises. His argument is that rather than finding an aggressor and defender, leaders on both sides assumed that their side was the defender, which complicates analysis within traditional deterrence theory. While leaders' assuming that opponents are acting from hostile intentions is not a new idea, Zhang does raise interesting questions about the broad applicability of standard deterrence theory to these (and potentially other) cases. What is needed, Zhang argues, is greater attention to the outlooks and perceptual frameworks of each side of the dispute, as well as attention to the sources of those frameworks. Attention to the impact of culture-bound perceptions is the offered solution to this problem.

I have a few disappointments. Several spelling errors detracted from the otherwise excellent writing in the text. More worrisome, many serious errors in quotations and references were found. My substantive disappointment is that there was insufficient attention paid to the "strategic culture" aspect of deterrence. Zhang's theoretical discussion provides a framework for the historical accounts, and its use adds depth and texture to the description of the dynamics of the bilateral relationship in these various incidents. Unfortunately, the discussion of the impact of different cultural, historical, and political phenomena on deterrence behavior is too limited. The final chapter touches on the impact of culture-bound perceptions by discussing each country's definition of security interests and perceptions of external threat and the impact of differing policymaking processes on each side of the dispute. Zhang notes that "the problem was not that the United States and China had different cultures, but rather that each nation's leaders ignored that fact" (p. 272). American leaders frequently ignored messages emanating from the Chinese leadership via the official press, dismissing them as mere "propaganda" when, in fact, many of the items were drafted by top leaders, including Mao. Chinese leaders were unable to understand the pluralistic nature of American government and tended to assume that speeches by pro–Chiang Kai-shek members of Congress, lobbyists, and military officials reflected "true" policy and that White House statements were mere "smokescreens" (pp. 266–77). By doing so, each side's views of the intentions and motivations of the other was skewed, adding to the difficulty of successfully achieving deterrence. Although not stated explicitly, Zhang's discussion suggests that much of our thinking about deterrence, as well as our different leaders' ways of viewing deterrent relationships, is very culturally bound. Zhang's richly textured discussion of the dynamics of the deterrent efforts on both sides provides a useful backdrop for further elaboration of these themes. One hopes that he or others will pick up the threads of analysis here and take this discussion further. He has made an excellent start.

University of Wisconsin, Madison ROBERT B. MCCALLA

Beyond the Soviet Threat: Rethinking American Security Policy in a New Era. Edited by William Zimmerman. Ann Arbor: University of Michigan Press, 1992. $37.50.

Beyond the Soviet Threat presents a diverse collection of essays researched and written prior to or immediately after the collapse of the Soviet state. Part 1, entitled "The Passing of the Soviet Threat," combines contributions on Soviet mass and elite opinion concerning foreign policy issues by William Zimmerman, Deborah Yarsike, and Allen Lynch with studies of the evolution of Soviet military doctrine and patterns of third world intervention by Richard Hyland Phillips, Philip G. Roeder, and Andrew Bennett. Part 2, entitled "Western Approaches to Security in a New Era," includes a study of the evolution of America's Soviet policy by Miroslav Nincic, a survey of loose models for interpreting the superpower relationship by Charles L. Glaser and Ted Hopf, and an interesting and broadly optimistic discussion by Paul Huth of the prospects for an alternative security order in post-communist Europe.

The essays are thematically and methodologically diverse. In most cases they have much that is useful to contribute, both as comments on the Soviet experience and as exercises in systematic political analysis. As a collection, the volume suffers from a lack of unifying themes and shared perceptions, and certainly does not do justice to its title. There is a considerable amount of interesting material and reflection here concerning Soviet politics and the Soviet-American relationship, particularly during the years of *perestroika*, but relatively little guidance for navigating in the radically diverse and highly unstable geostrategic environment that the Soviet breakdown has created.

The demise of the Soviet state represents a kind of radical discontinuity that social science models need to come to terms with more directly. The methodological approaches represented in the present essays, ranging from the use of limited public opinion surveys through relatively formal modelling of trends in elite perception and in foreign and security policy, could once be defended as a more rigorous alternative to the traditional techniques of "Sovietology." The constraints that made Sovietology necessary have now all but disappeared, but with them has gone the Soviet Union itself as an autonomous object of study. Though it is obviously unfair to expect too much from work completed before the Soviet collapse had been consummated, one can only regret the failure of the present essays to confront the implications of the Soviet breakdown more directly.

It would obviously be incorrect to conclude that the demise of "Sovietism" means a complete break with the past. But the issue of continuity cannot be managed, as the present volume is all too prone to do, by asserting that conclusions intended for the USSR are more or less equally valid "for Russia and the other Soviet successor states," or with the anodyne conclusion that "it remains to be seen" how transferable models of foreign policy interaction conceived during the Soviet period will be to a post-Cold War international environment. It is necessary to ask how and in what ways the legacy of the Soviet past will manifest itself in today's Russia in a more systematic and convincing way. On the level of international society, models that seek to look beyond the Soviet experience must probe the implications of the collapse of the Soviet federation, the emergence of interstate relations between Russia and the newly independent states on its littoral, and Russia's own new role in world affairs in ways that move away from patterns of understanding born of cold war bipolarity.

Some of the conclusions in the present volume point in this direction: Andrew Bennett's essay on Soviet interventionism poses a "learning hypothesis" that can be applied to post-Soviet Russian behavior. Allen Lynch, Charles L. Glaser and Ted Hopf all call attention to elements of continuity in elite thinking that are relevant to understanding Russia's emerging international priorities. Paul Huth points to global trends that have the potential to further the integration of the Soviet successor states into a more open and less violent international community. But occasional insights do not alter the fact that *Beyond the Soviet Threat* does not take us very far beyond the Soviet Union at all. New research agendas, new kinds of questions, alternative methodologies, more systematic data collection, and a patient culling of conclusions against the background of a situation that remains very much in flux will be necessary to overcome the limitations that make the present evaluation read more like a requiem for the past than a guide for the future.

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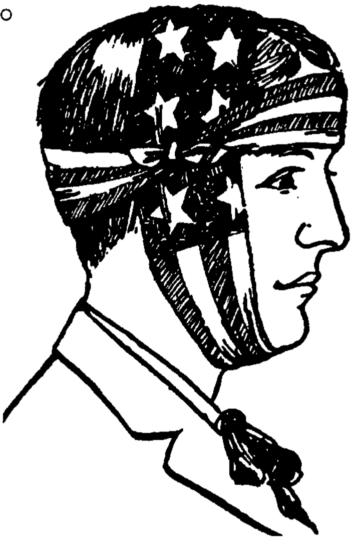

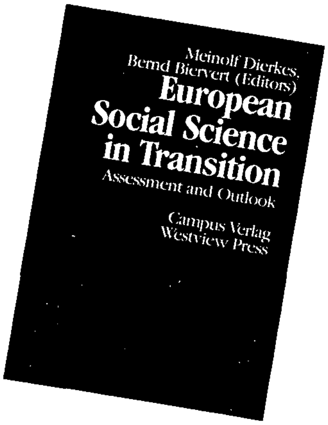

European Social Science in Transition
Assessment and Outlook

Meinolf Dierkes and Bernd Biervert, editors

Major Subject Interests: Sociology, Europe

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The volume concludes with two contributions on the role of comparative research in the development of social science in Europe, as well as on the general outlook for social science research in the future.

Nov. 1992; 640 pp. (tables, figures, notes, biblio); **0-8133-1629-4, $64.50 hc;** *A Campus Verlag Book*

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C. B. Macpherson was one of the leading political theorists in North America and perhaps the most influential voice on the left for a view of liberal democracy that was simultaneously sympathetic to its aspirations and critical of its achievements. His work provides the contributors to this volume with a common starting point from which to reflect upon the possibilities for critical perspectives on liberal democracy in light of the demise of its Marxist rival. The volume as a whole addresses the following questions: What (if anything) remains valid in previous left critiques of liberal democracy (including Marxist critiques)? And what new critical and constructive alternatives can the left offer to challenge the status quo? The contributors to this volume, from both the Anglo-American and Continental traditions, include Joseph Carens, William Connolly, Virginia Held, John Keane, Ernesto Laclau, William Leiss, Jane Mansbridge, Louise Marcil-Lacoste, Mihailo Markovic, Chantal Mouffe, Nancy Rosenblum, and James Tully.

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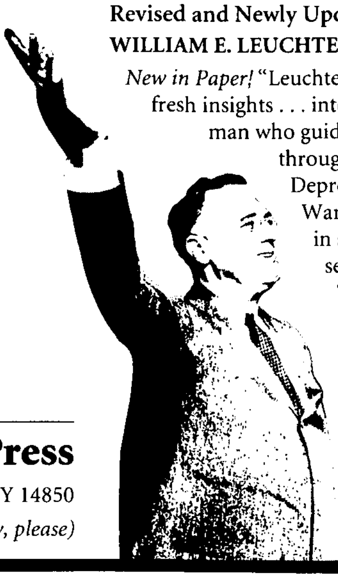

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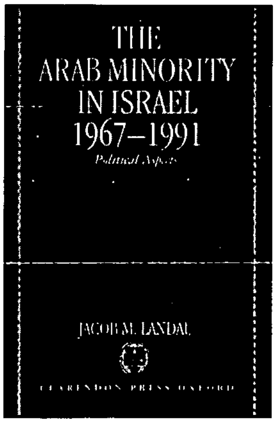

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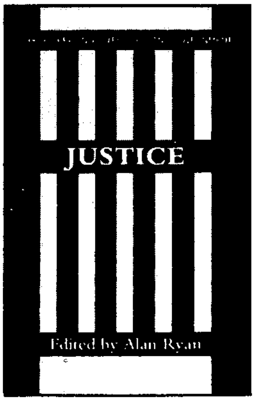

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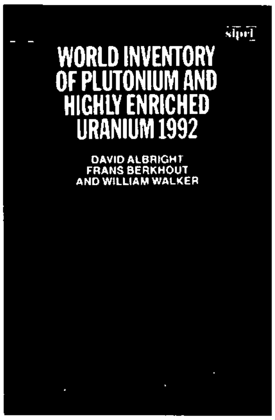
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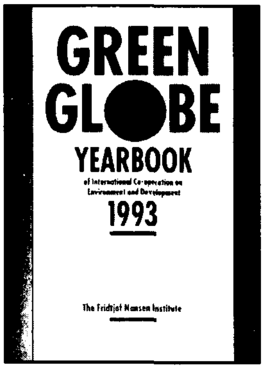

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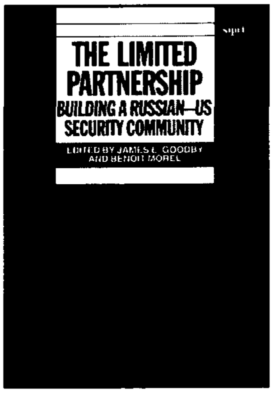
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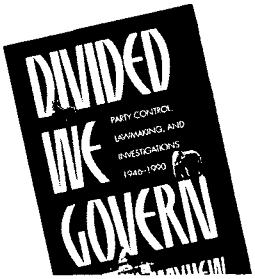

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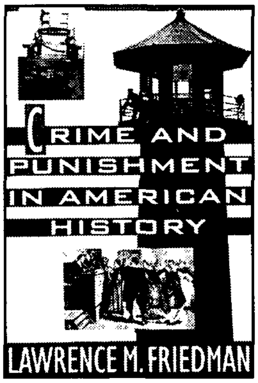

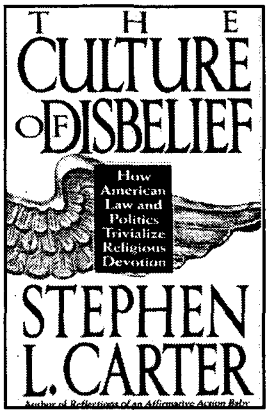
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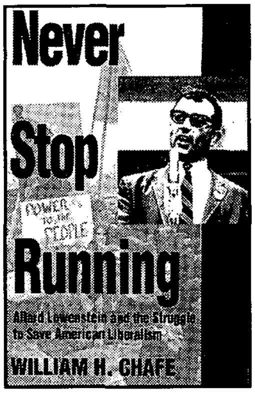
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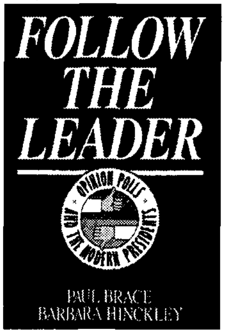
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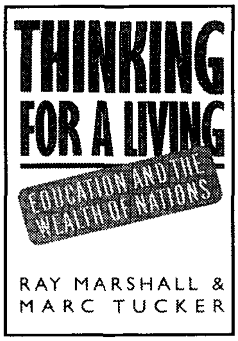
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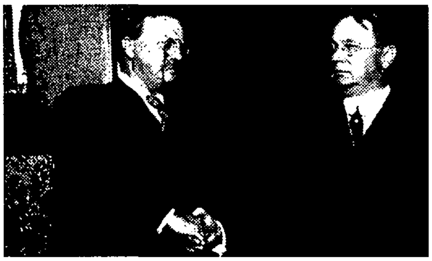

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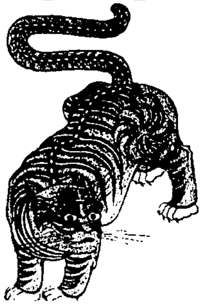

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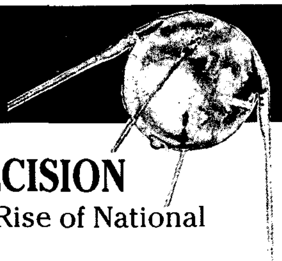

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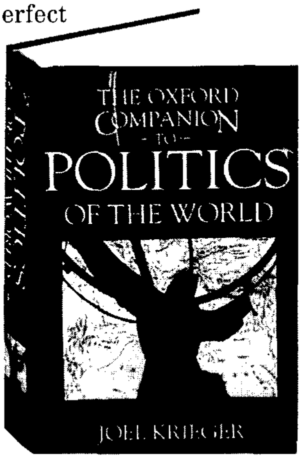

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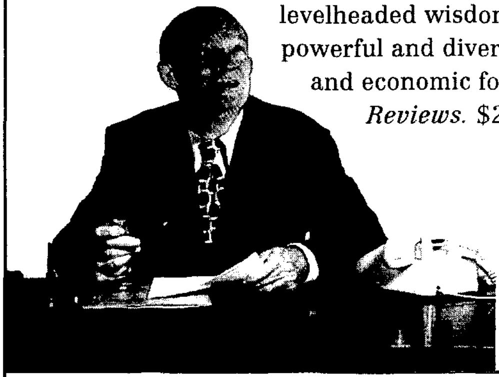

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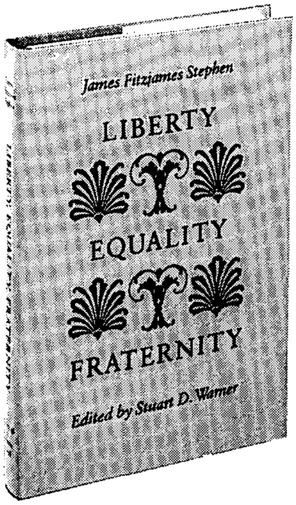

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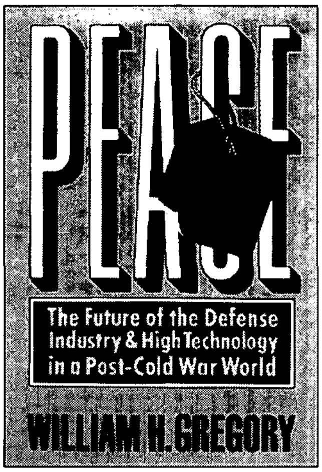
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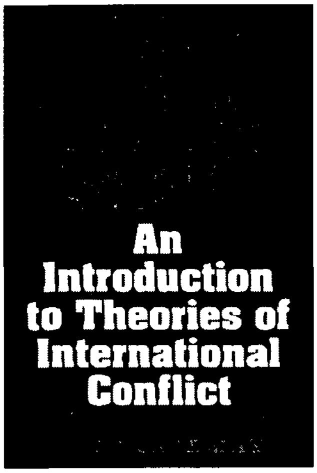

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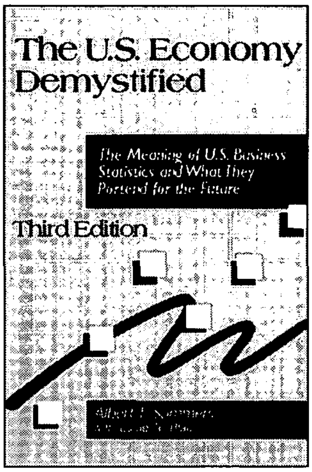
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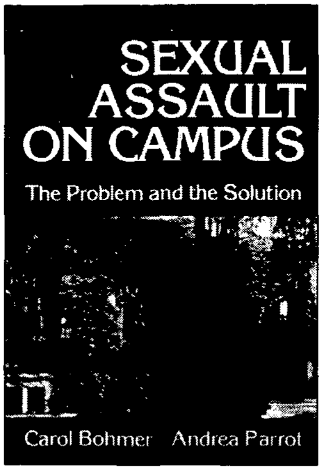

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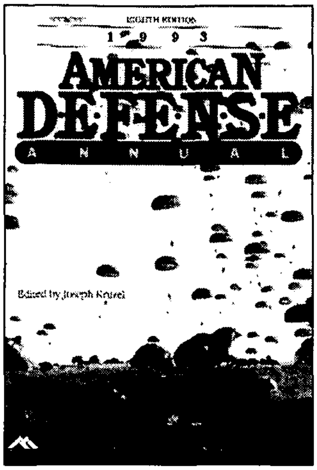

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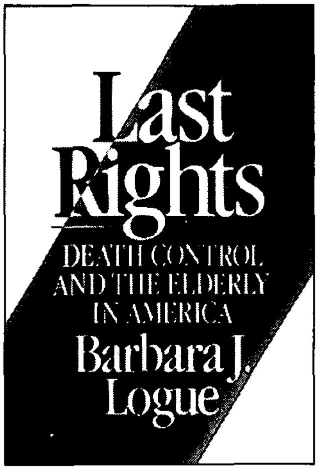

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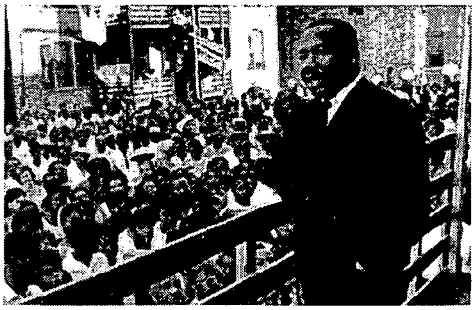

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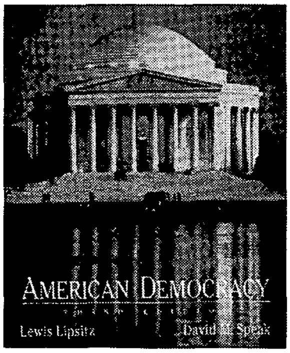
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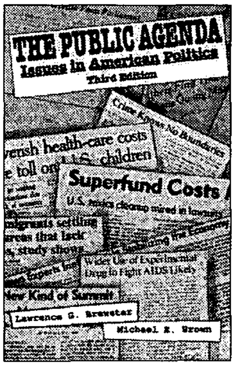
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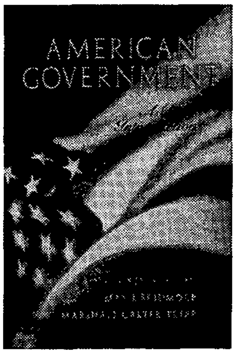
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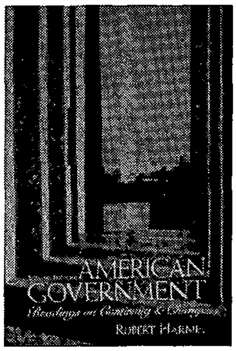
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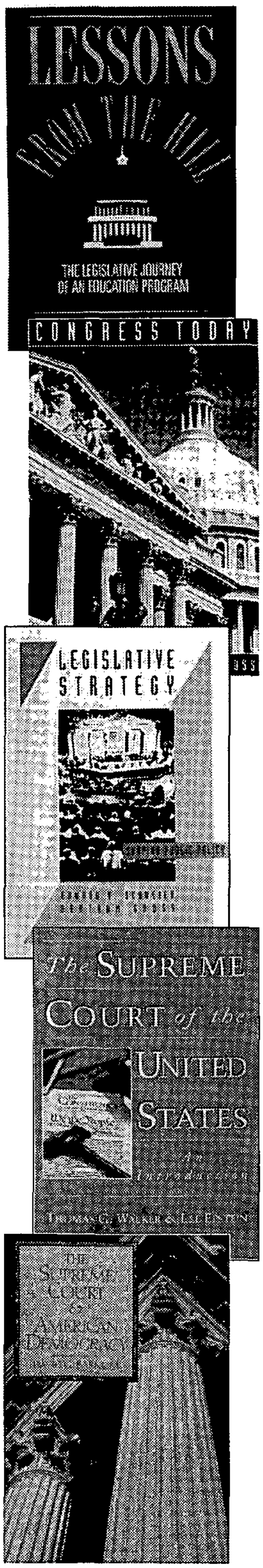
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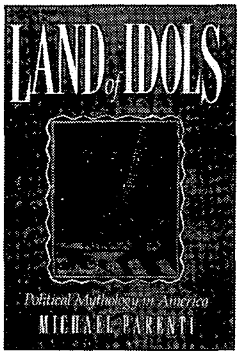

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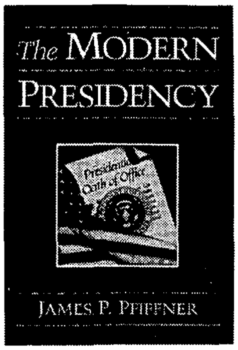

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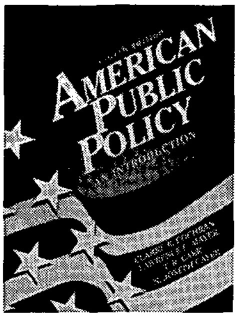

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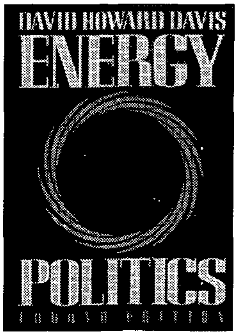

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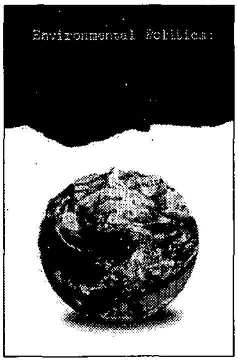

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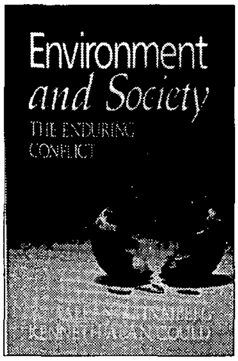

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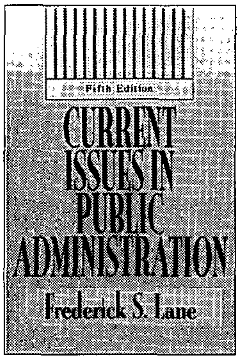

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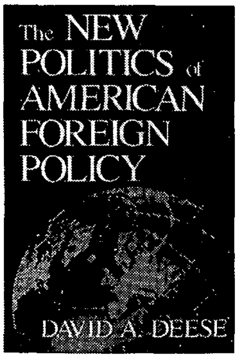

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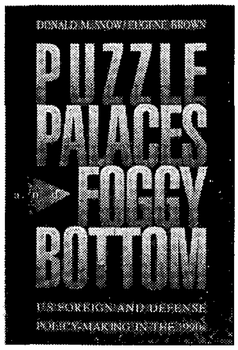
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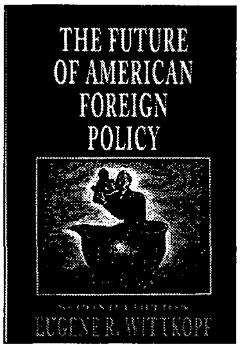
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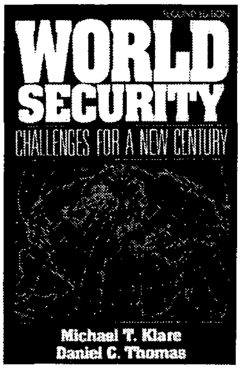
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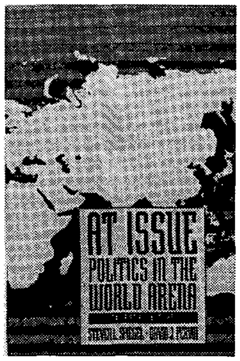
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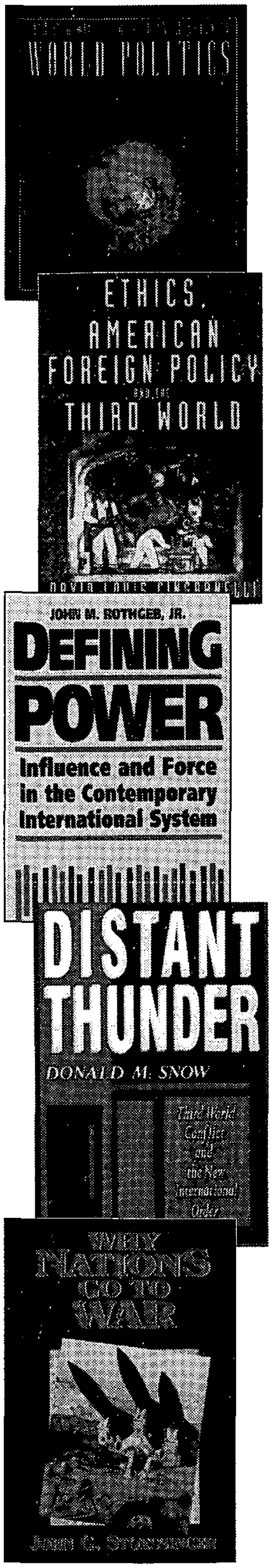
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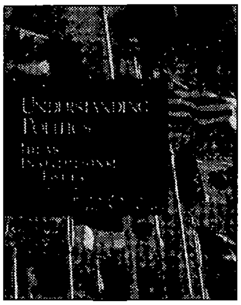

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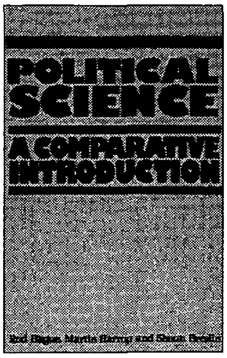

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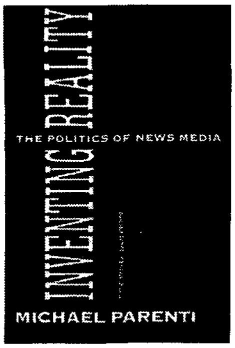

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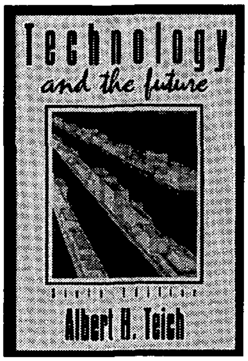

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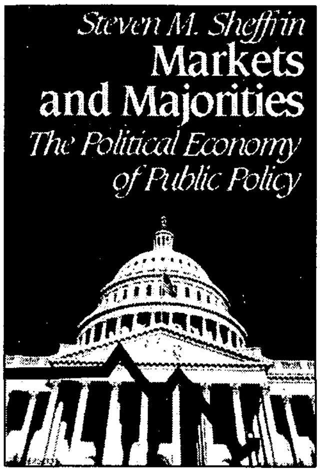

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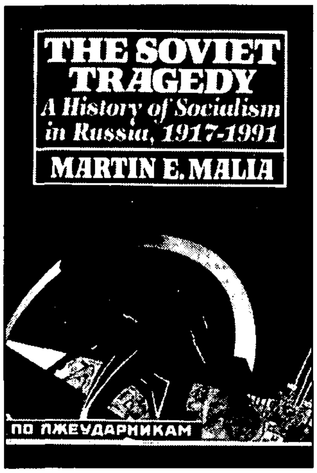

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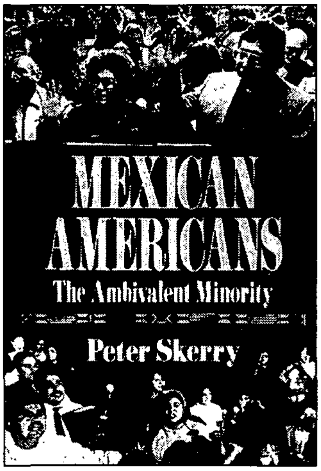

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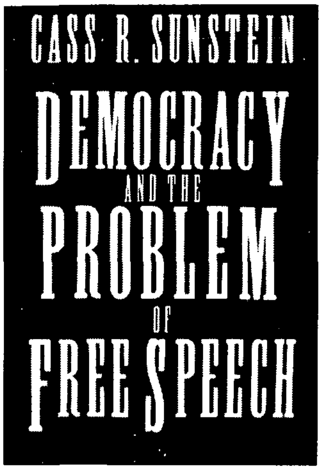

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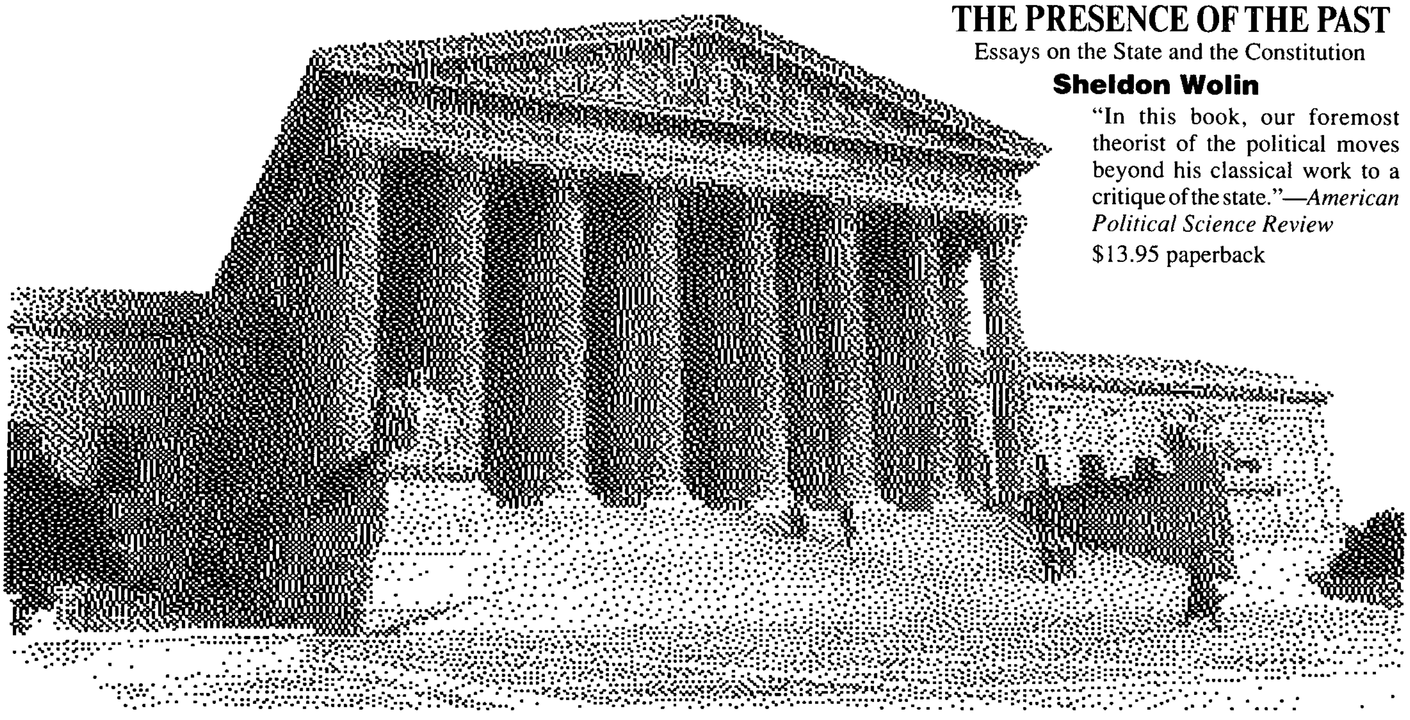

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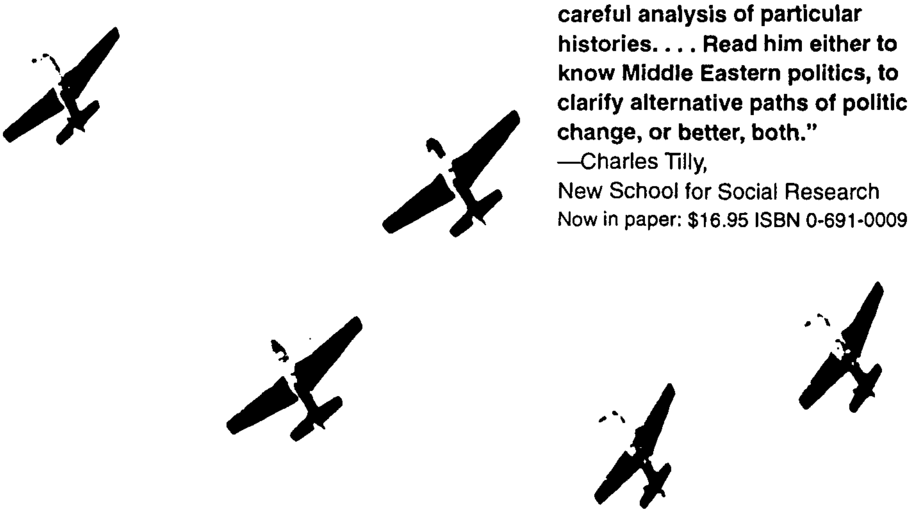

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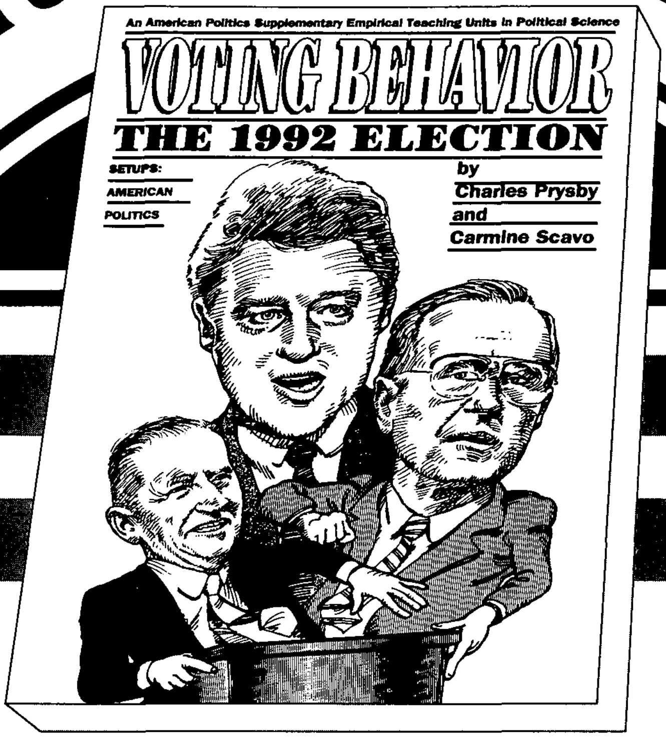

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